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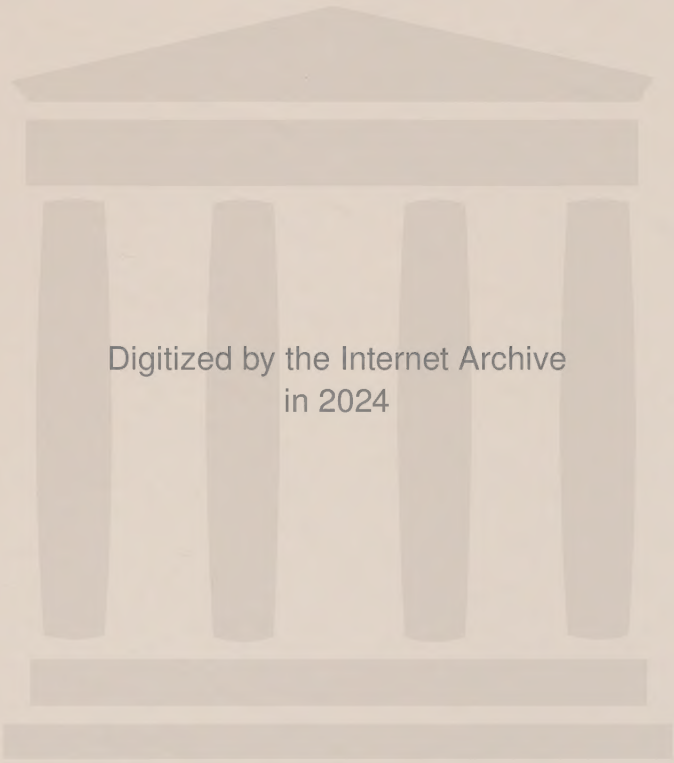
SCIENCE as POLITICS

Edited by Les Levidow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Les Levidow

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'. . . an association in which the free development of each is the condition of the free development of all'

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CONTENTS

Science as Politics 3

The Copernican Revolution: 7
Social Foundations of Conceptualization in Science
Norman Diamond

Socializing Darwinism: 38
Historiography and the Fortunes of a Phrase
Jim Moore

Dialectical Biology as Political Practice: 81
Looking for More Than Contradictions
Peter Taylor

Nuclear Politics in Yugoslavia 112
Les Levidow

Hazards for Export: Double Standards? 119
T. Jones

Sociology Pulls Its Punches 139
Tim Rowse

Niels Bohr: 'Mr Atoms for Peace' 150
Mike Barnett

Salvador Luria 154
Erwin Fleissner

Energy Futures 161
Les Levidow

Connections

Publications Received and Noticed 166
Notes on Contributors 173
Comradely Publications and Groups 174

SCIENCE AS POLITICS

Since the mid-1970s the Radical Science Series has demonstrated the non-neutrality of science, technology and medicine. Continuing that project, this collection illustrates the many ways in which political forces shape science and technology: the practitioners, the research questions, the conceptual frameworks, the funding institutions that promote certain directions, and the official histories of their progress.

☐ In every society, scientific world views embody some notion or other of the natural order, though what is considered 'natural' is often contested. In the case of the Copernican Revolution, which drew upon no new scientific data, Norman Diamond argues here that it implicitly abandoned earlier models of a fixed hierarchy; that is, it reinterpreted old data by reconceptualizing both matter and space. Diamond locates the social basis of that conceptual change in the changing class structure of sixteenth-century Europe. He goes on to suggest how people might become more conscious of their capacity for a revolutionary transformation of today's social order.

☐ When we see ideologues portraying competitive individualism as the natural order of things, we can easily recognize them as 'Social Darwinists': that is, those who pervert Darwin's theories by misapplying them to society. On the contrary, argues Jim Moore, the phrase itself is a misnomer, devised by various professionals intent upon sharply demarcating between science and politics. Arguing that such a distinction is false, Moore shows how Darwinism always was social. He also suggests how we might still use the phrase in order to criticize those who engage in Social Darwinizing today.

☐ Many critics have analysed the capitalist purposes and metaphors embedded in science, especially the mechanistic and reductionist models used in biological research. Two US scientists,

Richard Levins and Richard Lewontin, have been so bold as to propose an alternative model, 'dialectical biology', for which they see the greatest potential in Third World revolutionary societies. Here Peter Taylor evaluates their proposed model – as a way to investigate nature, and as a guide to political practice inside and outside the lab. He discusses how a dialectical approach might inform 'science criticism', that is, a challenge to the current political role of science, especially in the West.

☐ For many years, toxic industries have been relocating from major industrialized countries to the Third World. Obviously, one reason is the double standards in health and safety rules. T. Jones argues, however, that this explanation for hazards export ignores the deficient standards and enforcement in the exporting countries, where in reality no one is safe from mini-Bhopals. He shows how critics of hazard export overemphasize state regulation to the point of missing the importance of mass protest in obstructing destructive forms of industrial 'development'.

☐ Since the Three Mile Island disaster, and especially Chernobyl, the nuclear industry has become increasingly desperate to recoup its investment by exporting its technology to the Third World. Although located in Europe, Yugoslavia has recently begun a nuclear debate which reveals quandaries similar to those encountered by Third World countries. Les Levidow describes that debate, involving the relationship between industrial development and industrial democracy. In a separate article he evaluates proposals for Britain to adopt a socialist, non-nuclear energy policy, whose implementation would require a new kind of mass movement.

☐ When nuclear power was first inflicted upon the general population, governments presented it as 'Atoms for Peace', even though it provided crucial plutonium for nuclear weapons. Mike Barnett describes how the physicist Niels Bohr, having invented the notion of a 'peaceful atom', was cultivated by the nuclear establishment to promote it, while suspected of being a security risk.

☐ Salvador Luria, originally a socialist refugee from fascist Italy, went on to win the Nobel Prize for his pioneering work in the USA in the field of molecular biology. Erwin Fleissner describes

how Luria applied his political principles to methods of work and to public protest.

☐ Lastly, where do scientists' ideas come from? The sociology of knowledge has gone as far as to identify 'cognitive norms' – always historically contingent – by which scientists judge the truth of their theories. However, as Tim Rowse argues, this academic discipline holds back from connecting those norms to wider social forces. Its tunnel vision sees only internal factors while virtually ignoring the research and funding priorities of the military and big business.

Thus, in their different and related ways, these articles survey the many levels at which science is political.

Detail of 'The Last Judgement' by Hans Memling. Being weighed in the scale is Tommaso Portinari, manager of the Medici Bank branch office in Bruges, 1464–80. Formerly a flourishing merchant and textile city, Bruges was then in steep decline. Portinari typified the growth of speculation and shady practices that came with reduced opportunities for commercial investment. Having lost money and cheated the Medici, he was sacked. Memling's painting anticipates the year 1500 and the expected heavenly judgement to come.

THE COPERNICAN REVOLUTION
Social Foundations of Conceptualization in Science

Norman Diamond

This essay explores the social roots of concept-formation in science. Drawing on a case study, sixteenth century astronomy, it looks also at the nature of the social transition which the conceptual shift manifests. At the end, it attempts to redress the dialectical balance by reacknowledging the reciprocal influences, not only society on concepts but consciousness on social conditions, and turning more explicitly to the political implications of that reciprocity. It does so with the intention of clarifying the possibilities of a revolutionary transformation of our society.

At the outset, it is worth reminding ourselves of the political understanding that we have already developed regarding concepts and theories in science. In fundamental ways they are manifestations of the larger society in which they are produced and adopted. And they embody social premises at their very core.

That reminder is needed for several reasons. We must avoid getting preoccupied or diverted by sterile debates over the 'truth-value' of scientific concepts – that is, over the extent and ways in which they are true even though based on premises that derive from historically specific social configurations. In addition, the 'labour-process perspective', through which we have developed some of the social insights, risks being narrowed to look only at the immediate work conditions of scientists rather than at their entire social context (Diamond, *RSJ* 11).

And there is a more fundamental reason for such a reminder. Radical political practice depends on an understanding of the relationship between people's social surroundings and activities on

the one hand and their values, perceptions and attitudes on the other. Repeated political failures, from micro community organizing efforts to the macro level of attempted revolutions, have exemplified overly simplistic understandings of the ways in which each set of factors influences the other. Neither a determinism which expects consciousness to follow in automatic reflex from changed social conditions, nor a voluntarism which approaches people purely at the level of ideas irrespective of the social factors that condition receptivity, can underpin successful revolutionary strategy. The critique of science has gone as far in clarifying that subtle interrelationship of consciousness and social conditions as has any field of study/practice. Perhaps this is because bourgeois society presents concepts in science, more than in other spheres of thought, as abstracted from their social context, as least subject to social influences.

My particular case study suggests a slightly different perspective from other recent efforts in *Radical Science Journal* to raise some of the same theoretical and historical questions. A brief statement of the differences is appropriate before proceeding with the case study.

The differences centre on alternative ways to understand science concepts as mediated social products; and I consider all those that have found widespread acceptance among scientists, whether or not they have been discarded by later generations. I shall argue that they embody social premises. This is not the same as seeing them as aspects of a fully integrated social totality.

In looking for 'pure form abstractions' deriving from the social relations of production to account for concepts, Sohn-Rethel (1975) laid an important groundwork for a critique of science. His approach, however, rejects the social conditions/consciousness distinction, and thus the possibilities for political practice which are predicated on the distinction. For him, there is not a social environment separate from people's thought, which people incorporate into their thought. (If there were, that act of incorporation would have to be described as a process and as a relationship between distinguishable factors – perhaps with the possibility of intentional intervention.) Rather, his view is that people act and are therefore already permeated by the terms in which they act.

There is an important truth in this perspective, but it is too monolithic. People's thoughts are not always *per se* permeated by the terms in which they act. In the case of new sets of behaviour, there is a transition period. People may interpret their new actions, for instance their new participation in capitalist market relations, for a while in terms of an older world-view. They may act in conformity with new external standards while resisting internally. Different aspects of a given social configuration change and are changed at different rates. Useful as it may be as an analytic construct, there is never a wholly consistent social/mental reality. It is that lack of total consistency that creates political openings for us.

David Dickson's essay on seventeenth-century science (1978) more clearly illustrates both the possibilities and the problems in an unremitting holism. Dickson sees capitalism, and specifically the drive by capitalists to control the labour process, as behind the metascience, Baconian philosophy, which underlies science from that day till now. His essay is valuable both in identifying the key aspect of the social influence – how a surplus is extracted from the population, how labour is organized – and in seeing science as a manifestation of how the society is constituted.

This latter perspective, as with Sohn-Rethel, accurately makes the critique of contemporary science into a challenge to capitalism. Where it falls short, also as with Sohn-Rethel, is on the textured nature of historical transition, both social and in science. Looking only to the elements common to capitalism always – over a period of more that three centuries – obscures the transitions within capitalism, the times when there were and are opportunities to overthrow it. Similarly, it is not very helpful in understanding the changes in science that have taken place within that time-frame. A more helpful approach would require more concrete and historically specific social analyses.

There are advantages to choosing a case study from a slightly earlier period. In crucial ways, seventeenth-century mechanical philosophy builds on groundwork laid in the sixteenth-century. And yet it would be a mistake to see the earlier period as simply a precursor of the later, or Copernicanism as primarily an early manifestation of modern science. The era that produced Copernicanism was a distinct social stage. The transition from feudalism to capital-

ism was extended and complex. There was no rising class expressing confidence by making explicit its world-view. Science was not yet a means consciously chosen by ruling groups to pursue or solidify power, not yet a pointer to economic and technical development. Although control of the workplace was contested, science was not yet used to present things and people in a form amenable to the capitalist labour process. Rather, the premises that underlay sixteenth-century concepts correspond to what was a murkier social and economic context. Exploring those complexities provides a clearer sense of the relationship between consciousness and social conditions.

The following essay is based on a talk given a few years ago and distilled from a book-length manuscript. Its language is simple; it is meant only to be suggestive. The audience was primarily undergraduates, neither predominantly Marxist nor familiar with some of the theoretical debates. They were, however, students of social theory, reading Marx and Weber among others, and politically interested. Because of that setting and modest intent, notes are mainly substantive comments or parenthetical remarks rather than the documentation and references that would be expected in a more scholarly effort.

Concepts in the 'Evidence'

To understand any society that has hitherto existed, and its cultural products, the same basic questions need to be asked. Any hierarchically structured society, capitalist or not, needs to generate an economic surplus to maintain its elites. How that surplus is squeezed out of the underlying population, how people's work efforts are organized, what combination of resistance and social control shapes that organization of work: these are decisive factors. Needless to say, these questions require subtle and imaginative elaboration. I am not suggesting a cookbook with a series of recipes to be applied mechanically.

The issue is important, because how one identifies the key factors has political implications. Interested as we are in changing a society, it is necessary to know what sustains it and what changes must occur first in order to produce or make more likely yet others. In particu-

lar, the relationship between people's values, attitudes and perceptions on the one hand, and their activity and social environment on the other, is crucial. In other words, what is the extent to which the direction of a society can be shaped by changes in the consciousness of its members (and of which of its members) and to what influences is that consciousness responsive? Given the fundamental democratic transformation at which we aim, it is not only desirable but necessary to equip large numbers of people to analyse and to understand their society, the goal and their intermediate strategy.

Here is where 'imaginative re-enactment' enters, not only as a tool of historical analysis, but also as a means of persuasion. Altering someone's basic ideas is not a matter of presenting evidence; in its fundamental features it is a matter of empathy. People must first operate within the new conceptual framework, see what it feels like, before acceptance occurs. Partly this is because there is no such thing as 'evidence' that does not already derive from some conceptual framework. This is as true of science, and of how basic changes in scientific ideas come about, as it is of political ideas. Indeed, we can use science as a test case for how changes in consciousness more broadly occur.

When I say 'science', there are any of a number of widespread misconceptions that may come to mind. There are those of scientists themselves who think their work inductive: that an uncluttered look at raw data shows the relations inherent in the data, that the collection of facts gives rise to the underlying laws. I have already indicated a response: that there is no such thing as 'raw data', and that 'facts' derive their facticity only from being embedded in some pre-existing theoretical framework. In the historical illustration I shall use, the Copernican Revolution, Galileo praised Nicolaus Copernicus above all for being able to transcend the seeming evidence of his senses. As a side note, some philosophers who are aware that scientists unavoidably bring pre-existing conceptual frameworks to their investigations nevertheless argue that this delusion about the nature of their own activity is necessary for scientists to practise their trade. We can more readily forgive the philosophers' patronizing attitude, remembering how highly scientists are regarded in our society as models of critical self-awareness.

There are the misconceptions of social scientists who accept the

inductive misinterpretation and base their work on a vain quest to duplicate in their fields the methodology of a misunderstood natural science. One consequence is a restriction to trivial questions defined by the available technology – whatever can be quantified and run through a computer – rather than by any standard of importance. Lack of explanations and ideological biases, accepting prevailing frameworks because often unaware of their existence, are further results. There are also the misconceptions of certain kinds of positivist Marxists who treat science ideas as knowledge that is independent of and oblivious to social influences. Denying the social determinants, believing in an ever-closer approach to objective truth as science unfolds, this school has a predisposition to see a linear and inevitable progress in history. Here, also, the methodological and epistemological misconceptions have political consequences.

You have a sense already of my initial thrust, that broader presuppositions derived from people's social existence pre-define the scope of science inquiry and the range of available interpretations and solutions. Since I want to focus primarily on one set of changes in science concepts, it may help to look briefly at an unrelated example. You may know that Neanderthal Man was a nineteenth-century discovery, 1856 to be exact. It may have struck you as odd that in densely inhabited Europe someone had not come across this possible human ancestor before. In fact, the bones had been found and the caves explored for centuries. What was new and controversial was not the awareness of these bones, the 'evidence', but the interpretation. Species were thought of as immutable. Fossils that had been found for 300 years were not regarded as they are now, because that would imply that species can become extinct. This is a difficult perception if it is taken to mean that God is a poor designer and if your initial premise is God's perfection. Instead, bones not corresponding to the anatomy of existing animals, and fossils, were thought of as having been shaped by thunder or as the traces of animals that lived deep in the earth. An alternative interpretation was that they came from antediluvian animals, unrelated to any that had been on the Ark. The sense of the past was too brief to allow people to think of evolution; 5,200 years was about the most extended time-frame derived from the Scriptures in Western Europe.

These presuppositions – that people lived in a closed-in, sheltered world history of recent origin, overseen by a perfect God – conditioned all other interpretations. They had to stop making sense to significant parts of the population before Neanderthal Man could be discovered and before there could be general recognition of the possibility of evolution.

Looking ahead to the Copernican example, both comets and sunspots went unobserved during Copernicus's lifetime and in the previous few centuries. With the premises that the area beyond the Moon was the realm of perfection and that perfection implied immutability, translunar alterations were attributed to atmospheric distortion or, most often, were unseen. The astronomy of other cultures – particularly Chinese – that did not share those premises reported regular sightings of comets and sunspots during the same years. With the changing premises that made Copernicanism possible, there was a rash of comet and sunspot sighting in the latter half of the sixteenth century.

Given any set of premises, not only are certain phenomena not seen, but certain questions are not asked, certain possible investigations not undertaken. For yet another anticipatory example, Copernicus laboured at a severe disadvantage in trying to calculate planetary paths. Not until years after his death was it thought relevant to plot day by day the actual course of the planets. If one begins with the presumption that the heavens are perfect and therefore by definition orbits must be circular, then circular orbits are easy to ascertain. A circle is known, of course, if any three points on its circumference are known, or the centre and one point. Day by day observations, then are unnecessary. This is how 'facts' were and are constructed. Here again, discovering that planetary paths are not circular required a change in premises at a more fundamental level.

There is irony as well as faulty philosophy in the use of the word 'discovery'. 'Reconceptualization' is a more accurate description than 'removing the cover' and finding the unitary truth that has been waiting to be recognized. 'Evidence' is not 'out there' waiting to be seized. What is accepted as evidence is dependent on conceptual frameworks which are in turn shaped by what Weber called 'world-views'. 'World-views' are the most fundamental level of consciousness, ways of making sense of our activities and surroundings.

In other words, they are responses primarily to our social settings and struggles. There is no direct relationship between our ideas and social conditions. But our social existence creates the possibilities and sets the limits for the ideas we do have by shaping the world-views which underpin all our thoughts.

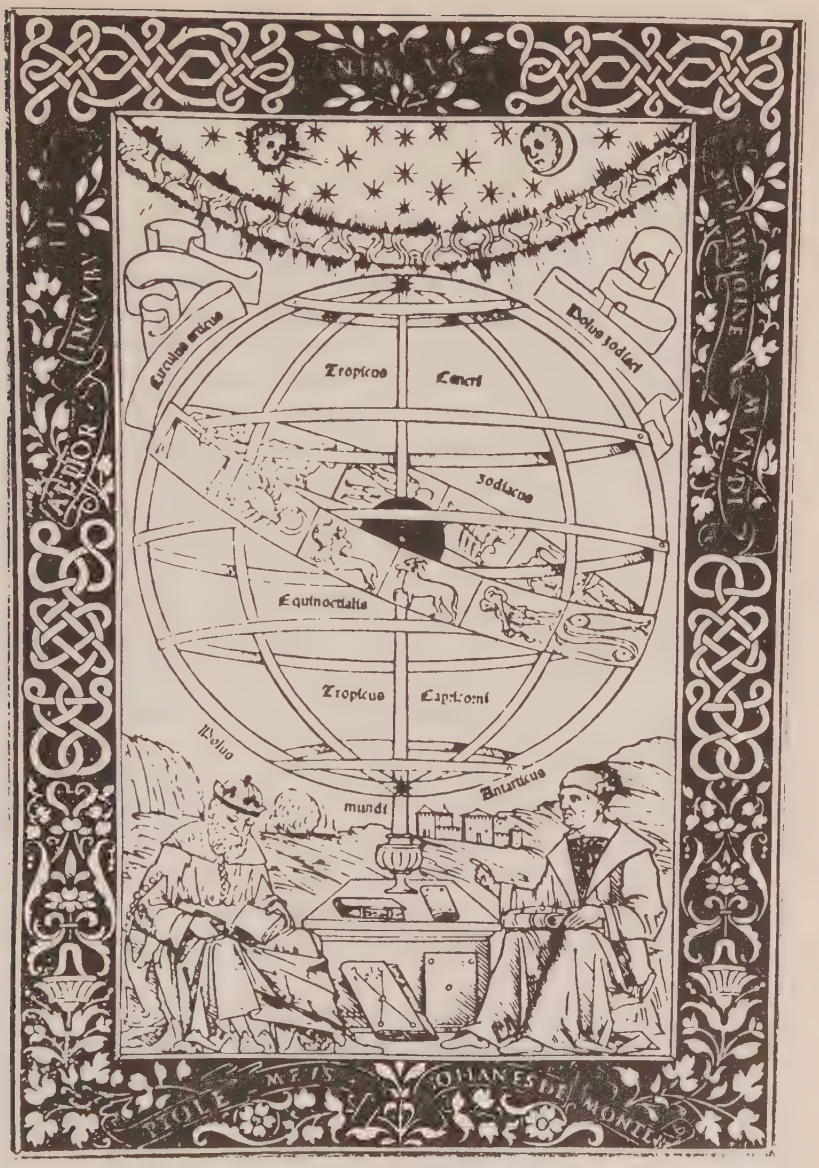
Copernicus versus Ptolemy

In looking more closely at Copernican astronomy and its predecessor, the first task in explaining why one replaced the other is to identify the world-views, the premises most often not even recognized by the people who hold them. Having done that, we can then attempt to account for the changing world-views in terms of changing social conditions. Let us look first at the astronomy itself. Simply from a surface description, see if you can discern an underlying metaphysics.

The outlines of the story have often been told. Before Copernicus, astronomy consisted in the calculation of celestial positions. Perfected by Ptolemy and refined ever since Ptolemy wrote in the second century, the accepted theory divided the observed paths into a series of interrelated circles, much as a machine in a planetarium might consist of multiple gears of different sizes, rotating at different speeds, turning in different directions, tilted at different angles. Sometimes the circles were designed to be off-centre, sometimes to rotate at a uniform rate only with respect to yet another off-centre point. The largest circles travelled around the stationary Earth, located at the centre of the universe. These variants of circles could be modified to yield any path, any geometric shape. As Copernicus said, 'by connecting and combining regular motions in various ways they could make any body appear to move to any position.'

This sky geometry was remarkably accurate, well within the capability of the naked eye. It is important to note that this was not a dynamics, nor was it an ontology. There was not a claim to *explain* heavenly movements, nor was there a statement about the reality of those movements. Astronomy was a series of devices for purposes of calculation only.

In uneasy association with that astronomy was a cosmology of mixed Aristotelian and Christian origins. Here is where explana-



Ptolemy's *Almagest*, originally published about 150 AD, then translated from the Arabic in 1175.

tory efforts and aesthetic considerations entered. Surrounding the Earth were concentric crystalline shells, one each for the Moon, Mercury, Venus, the Sun, Mars, Jupiter, Saturn, and furthest out, the sphere of the fixed stars. These all rotated by their friction with each other, having received their initial impulse from the Prime Mover. In a more updated understanding, angels pushed each orb. The shells were composed of a unique celestial material, 'quinta essentia', with properties unlike those of the four terrestrial elements. There were incompatibilities, at least that we see, between the astronomy and the cosmology – for instance, how to plot celestial paths given these substantive shells? However, they rested on certain premises in common and, in a way reminiscent of contemporary academic specializations, astronomers did their calculations, philosophers did their cosmologizing, and the combination was generally thought of as one theory.

Copernicus, of course, juggled the sky geometry. In his understanding, the Earth moves, and it does so around the Sun. Therefore the Earth is one among the other planets. There is one other important feature that is new in Copernican astronomy. He broadens the scope of astronomy to include cosmology. His concern with reality, not simply devices for calculation, leads for the first time to a *system* of astronomy. For Ptolemy, each problem was dealt with in isolation. For Copernicus, any slight change affects the entire integrated system. As a result, for Ptolemy, no single planetary problem ever required the use of more then six epicycles at one time. Calculations for Copernicus are far more complex: several planets have to be considered at once. For the rest, Copernicus uses basically the same geometric devices as Ptolemy, multiplying deferents with epicyclets on epicycles on eccentrics, all those different kinds of circles and centrings.

The two theories are formally equivalent in terms of what the old astronomy claimed to do; that is, they are geometrically equivalent. The triangles formed by the Earth, the centre of the large circles, and the planets in epicycle equal the triangles formed by the Earth, Sun and planets. There is no positional observation that can validate one theory at the expense of the other.

In the choice between the two approaches, common sense seemed

to argue for the Ptolemaic theory. The Sun rises, after all. Were the Earth to rotate at anywhere near the speed necessary to account for celestial movements, it was thought that centrifugal force would tear it apart. The planets and stars, having to spin so much faster because further out, can do so safely because made of a heavenly material not subject to terrestrial laws such as that of centrifugal force. In any case, were the bulky and heavy Earth not at the centre of the universe, it would quickly fall there.

If that does not seem commonsensical to you, that is because you do not share the premises on which that common sense is based. Common sense, like language, is a social product and varies with how people live.

Let us return more carefully to the underlying metaphysics. In the old world-view, nature was discontinuous, reality was split. What we think of as 'nature' was divided in half, with different laws for phenomena above and below the lunar sphere. Down here was the world of change and imperfection, out there the world of ethereal harmony: here buzzing confusion, there motion in perfect circles. We *could* know that the heavenly paths must be perfect, but the laws of their actual movements were beyond human capability. In a poem of disputed authorship but attributed to Galileo, the Aristotelian straw man, Bozzio, recommends:

> and learn from him who for onlookers draws
> the diverse movements and aspects of each star
> while keeping to himself elect thoughts of the cause.

Ptolemy explicitly denied that the planets actually could move in those circles on circles, as it was necessary to imagine them doing in order to calculate their positions. there was a despair, a renunciation of the possibility of attaining truth. Moreover, it was the motions of the heavenly orbs which produced change on Earth. Astronomy was in many respects simply an adjunct of astrology. Human knowledge and human control were premised as being severely limited. On the other hand, terrestrial nature was immediately and fully intelligible: qualitative categories fitted unaided sense experience. Things were as they seemed; different qualities were necessarily embodiments of different substances. Ice was something different from steam; heavy was qualitatively distinct

from light. There was neither continuity nor relativity. To treat ice and steam as differences of degree rather than of kind, or to treat phenomena mathematically, would require a sense of reality being distinct from appearance, a sense of different qualities being somehow reducible to the same substance.

The universe, then, was a fixed hierarchy, from the Empyrean, the dwelling-place of God, the seraphim and the cherubim, through the shells, all the way down to Hell. People were intermediate, part soul, part clay, bounded in time and space, with restricted capacities, and with the reassurance that somewhere out there was a realm where everything went smoothly and change was absent. In other words, the pre-Copernican facts of nature contained premises from pre-Copernican society.

The science of that time – like all science and all thought, modern as well as medieval and ancient, mathematics, physics and astronomy as well as the biological and social sciences – was conditioned by the underlying world-view, by those premises which are far from explicit if one simply reads the astronomy or whatever. It is necessary to dig deeply to get at them. This is even more difficult with modern science because the premises, the metaphysics, are so widely shared and seem so obvious and natural that they become nearly invisible. The criteria and possibilities for advance and the limitations in the science are set by these premises.

The New Cosmology

Even the best commentators sometimes miss the fundamental shift in world-view that makes possible Copernican astronomy. Take for example Chapter Four in Book One of Copernicus's major astronomical work, *On the Revolutions of the Heavenly Spheres*. Chapter Four is entitled 'That the Motion of the Heavenly Bodies is Uniform, Circular, and Perpetual, or Composed of Circular Motions'. It begins by presenting the author's dynamics, his explanation for motion, which is that spheres rotate by their very nature. Then he argues that heavenly bodies must move circularly, or in paths compounded of circles, because their positions recur and are subject to definite laws, and only a circle can restore the place of a body. Therefore heavenly motions only *seem* irregular, raising the possi-

bility that the Earth is not the centre of the circles on which these bodies revolve.

In a book which is still one of the best on the subject, Kuhn's *The Copernican Revolution*, the author comments, 'So far every one of Copernicus' arguments is Aristotelian or scholastic, and his universe is indistinguishable from that of traditional cosmology.' He is dead wrong. For Aristotle, movement was a property of celestial bodies as substance, not of their forms. Copernicus has eliminated the unique celestial material. For him, there is continuity of matter, not a divided nature. Consider Chapter Two, in which he holds that the Earth is spherical; by this point in the argument not only has he laid the groundwork for the Earth moving, but also is assuming what had been unthinkable: that the same physical laws are applicable for the Earth and for the heavens. Underlying this entire argument is a rejection of the old notion of a fixed hierarchy.

The editors of the best English translation claim in a footnote that Copernicus in this chapter contrasts the heavens with the corruptible Earth, 'unable to free himself' from Aristotelian conceptions. On the contrary as we have seen, he has broken in decisive fashion with his precursors. To reinforce their view, they anachronistically translate the Latin 'orbe' as 'body' and 'orbit', rather than as 'circle', thus denying Copernicus's belief in the reality of spheres and circles. Translation, as common sense, often incorporates premises of which its authors are not conscious. Copernicus has retained the old vocabulary and imagery just as he used the old astronomical observations. What is different is the world-view, and so even the old imagery takes on new meanings.

Copernicus never met the old common sense head-on. In response to the older certainty that the Earth would fly apart were it to rotate, he simply repeats his arguments: rotation is natural to a sphere and therefore not a destructive motion, and anyway the heavens could even more so be expected to fly apart under the old cosmology, being supposed to rotate so much faster. He has simply assumed what was so decisively rejected in the old cosmology: that sublunar and translunar spheres have the same physical properties. Similarly, in intellectual exercises proving that the Earth could not possibly move, theoreticians of the old cosmology had pointed out that movement by the Earth would result in apparent displacement

of the relative positions of the stars. The only alternative was that the universe was so large and the stars so far as to make this parallax unnoticeable. However, to imagine the universe that large was clearly absurd. Copernicus never attempted a direct reply. Indeed, the predicted parallax was not observed until the nineteenth century. He simply asserted that the universe might be quite a bit bigger than had been thought for centuries. His arguments could not and did not persuade Aristotelians. Basically, he presented a new belief system.

Beneath many of his innovations is a new attitude towards hierarchy. For reasons we shall see, the old understanding of hierarchy no longer made sense. This is manifested in many ways: in his dynamics, with the same motions for terrestrial and celestial bodies; in the comparable laws that apply to what had formerly been separate and incomparable realms, as in his discussion of gravity (for Copernicus, the natural tendency of a body to assume a spherical shape); in his denial that what had been thought of as higher spheres have any authority over lower; even in his combination of astronomy and cosmology, practical and theoretical pursuits. He laid the groundwork for terrestrial knowledge and concerns to be raised to what had been the exalted level of theology. For Copernicus, then, there are not materials and laws and realms beyond human ability to understand. Explanation is explicitly his goal. With the Earth now a planet, there no longer are external determining forces. Much more is sensed to be within human control.

As well as a new attitude toward hierarchy, there are other components in the newly developing world-view. I would like more briefly to sketch some additional major changes in fundamental premises that make possible the new astronomy and are ways whereby Copernicus and some contemporaries made sense of their changed social reality.

Reality itself, no longer split, is *mathematicized*. Geometric constructions, formerly calculation devices imposed on the heavens, now are seen as the essence and component of material reality, because that reality is qualitatively equivalent. Further, with the Earth now the same as the heavens, it is possible to use mathematics to study terrestrial phenomena.

There is a *particularization*. The predominant view had seen ob-

jects as one of a class, determined by the telos, the natural end, of that class. In Copernicus, it is necessary to analyse the different forces operating on each object. There is a sense of the uniqueness of individual experience within a framework of common rules.[1] Part of the basis of the mathematicization is an attempt to find what is in common among entities newly seen as distinct.

Of fundamental importance, *seeming* is not *being*. Reality is different from appearances, and an ontological thrust must penetrate beneath surface phenomena. Appearances, as we know, are *changing*: there is a 'buzzing confusion'. With an end to the distinction between sublunar and translunar realms, there no longer is an area of perfection where change does not occur. The whole universe is opened to change. The old reassurance is relinquished, and change is accepted (which is not the same as being welcomed). The reality behind appearances remains calculable, ever returning in a circle, harmonious. This was a major emphasis and major attraction for Copernicus.

Harmony is manifested in a *systematizing*, tied to the concern with physical reality in the astronomy and with explanation and not merely description or prediction. Copernicus begins with the goal of presenting an integrated system with no arbitrariness. Some of what was only *ad hoc* in Ptolemy is entailed in Copernicus, for example that the planets be brightest for an earthly observer when retrogressing.[2]

Nature is no longer immediately and fully intelligible: what causes what, has altered. Implicitly there is a new dignity to nature. Terrestrial concerns are raised to the level of celestial, and human concerns are part of the natural.

The logic of Copernicus's argument leads to a radically new attitude towards nature. Quantification can facilitate human domination of nature, as with Bacon in the following century. However, that attitude is not in Copernicus himself. His is a gentler intention, learning about nature to be a better part of it and to work better with it.

These elements of a world-view, sketchy as they have been, are not explicit in Copernicus. A careful reading shows that he presupposed them and that his insights are impossible without them. What makes them a social phenomenon and suggests the necessity

of a social explanation is that they are beginning to be found at the same time in other authors and in quite different realms of thought. Copernicus's other writings, on subjects far removed from astronomy, exhibit the same conceptual framework, so much at odds with the presuppositions that formerly had prevailed among intellectuals. In fact, this new world-view enables him to be innovative on other subjects as well, such as the circulation of money. We shall see shortly that the bases on which Copernican astronomy was accepted – and this after all is the more fundamental question than that of the individual creativity of the great scientist – cannot possibly be accounted for by technical factors, internal to the astronomy. A social explanation is necessary.

Interestingly, Copernicus was not alone in the direction of his astronomical thinking. There were a number of others working along parallel lines, with similar insights possible only with some approximation to the new world-view. I certainly do not want to downgrade Copernicus. He was the only one methodically to work out the mathematics of those insights – an incredible task that took him thirty-plus years, with bad observational data. However, the approach I have adopted here suggests a major part of an explanation for the common science phenomenon of multiple independent simultaneous discoveries. As societies change, the impact is different on different groups within the society. Old premises no longer make sense to particular groups. New world-views are developed as new explanations of people's changed situations. These new world-views make possible and stimulate new approaches in all realms of consciousness.

Society in Transition

The world-view which underlay Copernican astronomy was by and large a response to developments in North Italy. There, with a history and social structure that distinguished it from most of the rest of Europe, many of the decisive changes that were also beginning to affect the west and centre of the continent had already been manifested. Copernicus's own background, as a product of the fading frontier society of Central Europe, meshed with what he saw and heard as a student in North Italy.

Before getting to the particulars, some general remarks are called for. It is not surprising that a historical time which has been called both the 'Renaissance' and the 'waning of the Middle Ages' should exhibit a multiplicity of different ideas and world-views. Different groups within the society were put under different kinds and different degrees of pressure by the underlying changes. The different outlooks corresponded to their different experiences. Some of the struggle in the realm of ideas and outlook was part of the conflict between different classes and social groups.

Social transitions are never sharp. Even in revolutions, people do not transform their lives instantaneously or begin with blank slates. Sometimes, the reaction of some social groups is to hold on all the more tightly to what they had or now think they had. An outlook that no longer relates, in substantive terms, to people's lives may still provide reassurance that things are not changing or at least that 'we're still the same'. Living the transition, the outcome is not clear. Keeping in mind that our own is an extended age of transition may help you to empathize with Copernicus's time. It is a historian's truism that all ages are ages of transition, but in so far as it refers to people's awareness, it is a truism that is false. Most often in most societies hitherto, people have simply taken their society as given. This was less true than usually in Copernicus's time, but that does not mean that people were hopeful. Not only intellectuals but peasants and urban artisans interpreted their age as an ending of an old way of life. There is abundant evidence of this in the millenarian movements and widespread conviction that the year 1500 would mark a decisive change.

In the two key geographical areas for Copernican astronomy, the predominant sense was one of decline. This was a time when earlier opportunities, economic and political, and opportunities for social mobility, were disappearing and physical frontiers were narrowing. Many of the possibilities created in late medieval society, possibilities important for the future, were being at least temporarily suppressed. For a while, 'liberty' had been an important vision and catchword. Guilds, movements against guild leadership from within, and lower class movements outside of guilds, had been important, sometimes predominant, pressures in civic affairs. Now the politically active free cities were falling, their markets and pro-

duction base undercut. Christianity was shrinking with Turkish advances. Italy itself was dissolving in anarchy and conquest.

There were few clear sets of values, no identifiably rising classes, and little class confidence. This was the time when the Medicis made huge fortunes on usury during their lifetimes, and fearfully restored the money on their deathbeds. The Church sold indulgences – forgivenesses – bowing to and taking advantage of conflicting values and practices. Here, incidentally, is some of the origin of that strong and new sense that reality is distinct from appearance, that sense which is so important to the political and social thinking of the time. It was manifested as diversely as in the growth of etiquette, where how one appeared was recognizably different from how one really was; in standards of marital fidelity, which changed significantly in a manner parallel to the new etiquette; and explicitly in Machiavelli's political thinking, as well as being basic to the new science.[3]

The cultural flourishing that we know of as the Renaissance was grounded in a long-term stagnation and decline. Marx's metaphor for the transition from feudalism to capitalism – 'the new society growing within the womb of the old' – may be misleading. At the very least it is important to recognize that the labour was hard, the gestation extended, and the character of the offspring by no means predetermined. Rather than a transitional period, this time is best understood as a stage of its own with widely varying possibilities for future direction.

There had been an earlier commercial boom in the twelfth and thirteenth centuries. New groups developed, centring on merchant capital. They had new sources of wealth, new bases of power and

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Opposite: Title page of the first edition of Vesalius's *De Fabrica corporis humani* (*On the Fabric of the Human Body*), Basel, 1543, published the same year as Copernicus's major work. Vesalius's presentation here, and what we know of his historical practice, was strikingly different with respect to hierarchy from what prevailed in anatomy studies for centuries. Previously, demonstrations were performed to ranks of students arranged in tiers or loges; the lecturer spoke from a raised lectern removed from the body being dissected and gave directions to the demonstrator, who pointed with a sceptre, in turn guiding the work of the barber, who did the actual dissection.

ANDREAE VESALII
BRVXELLENSIS, SCHOLAE
medicorum Patauinæ professoris, de
Humani corporis fabrica
Libri septem.
CVM CAESAREAE
Maiest. Galliarum Regis, ac Senatus Venet. gra-
tia & priuilegio, ut in diplomatis eorundem continetur.

new sets of values. There was direct conflict over control of the labour process between merchant capital and semi-autonomous artisan workshops. In some places the older, rigid social hierarchy was replaced by competing hierarchies and competing visions of hierarchy. Brecht, through a ballad-singer in his play *Galileo*, captures the relation between the older astronomy and the older fixed hierarchy, still more prevalent in most places in Europe than in Italy.

> When the Almighty made the universe
> He made the earth and then he made the sun.
> Then round the earth he bade the sun to turn –
> That's in the Bible, Genesis, Chapter One.
> And from that time all beings here below
> Were in obedient *circles* meant to go:
> Around the pope the cardinals
> Around the cardinals the bishops
> Around the bishops the secretaries
> Around the secretaries the aldermen
> Around the aldermen the craftsmen
> Around the craftsmen the servants
> Around the servants the dogs, the chickens,
> and the beggars. (Scene 9, emphasis mine)

That outlook lingered and retained institutional support even though, for a while, a much greater social mobility was possible. There was a time in the fourteenth century about which a contemporary French historian has written that peasant standards of living have never since been higher. Movement to cities also represented a form of social mobility, as did the opportunities for new wealth. Increasingly, also, some of the incipient states advanced commoners to high positions, as kings created a social base separate from a nobility they sought to subdue.

The late fifteenth and early sixteenth century, Copernicus's own time, was one of regression. In North Italy there was an extended economic slump. Growing public debt gave control of the cities to richer families. There was a social alliance at the top of the formerly competing hierarchies, as wealth made in commerce went into land purchases. Rather than reorganizing the agricultural labour process, new owners simply pressed the peasants harder. Old feudal

customs were resurrected, as these bankers and wool merchants held tournaments and jousted in front of their fair ladies. Where industrial capital was important, especially in Florence, it was stymied by the inability to obtain a sufficiently large and secure workforce. The city-state could not expand the territory it controlled and thus draw in more workers, in part because creating an army from the ranks of the citizens was seen as risking too much power in the hands of the lower classes. As a result, capital increasingly left manufacturing for land and finance, with the same results as elsewhere.

Earlier opportunities for social mobility were spoofed as chimerical, as in the folklore story of a peasant girl on her way to market, carrying a basket of eggs on her head. As she walked, she daydreamed. First she would sell the eggs and buy more hens. Then, with the larger supply of eggs thus generated, she would buy and raise some pigs. Selling them at market, she would begin a cattle herd. In her fantasy she became a very wealthy woman, eventually marrying the prince. Here she bowed her head at the splendour of her dreams come true ... and all the eggs fell out and were smashed.

The hierarchy was re-rigidifying; gulfs between classes were becoming more fixed. The alliance between merchant capital and feudal elites suppressed challenges from below. Etiquette developed as a code of conduct that enabled a clear distinction and a feeling of superiority on the part of the upper classes, and a reminder of what they had in common. It was a product of that consolidation at the top of the hierarchy. This was also the time in which gourmet cooking developed, albeit first in the part of France that bordered North Italy. Previously, upper classes were distinguished in their eating habits more by quantity than by any qualitative standards. Here too, the lines between classes extended in their starkness from the writings of social philosophy into daily life.

Opportunities were tightening; yet, on an individual basis for offspring of groups that had already made it, there were still vast possibilities. It is interesting to see how the metaphors for 'fortune' changed. The early medieval wheel of fortune, ineluctably revolving, beyond human control, was followed for a while by a sense that people could accomplish anything. By the early sixteenth century, the metaphors are more ambiguous, the judgement less certain: for-

tune is a sailboat on the high seas, or a woman who must be dealt with forcibly. There is a wilfulness in fortune, certainly more than a passive object of our attentions. Still, there was the sense that much more is within human capacity and control than when people had been thought of as the passive objects of external forces.

Where the contribution of northern Italy to the premises of Copernicus is greatest, I think, is in the insistence that what is real is not empirical phenomena but is a deeper structure of mathematical relations. There have been attempts to explain this as somehow an outgrowth of the new commercial use of arithmetic that developed with the extension of trade; however, these attempts are dubious. The arithmetic was elementary and unskilled; mistakes of addition were the rule, not the exception, even in the budgets of states.

A closer relationship might exist in the abstraction of the mathematics and in the attitudes of systematization and calculability engendered by the commerce. However, the sense of reality as mathematical has a more specific and more mediated source that influenced Copernicus; it stemmed from social relations, but not directly. It came from the Neo-Platonism of the humanist philosophers of North Italy. A generation or two before, these were the people who would have been active politically in the city-states. In the late fifteenth century, above all in Florence with the growth of the Medici autocracy, the humanists withdrew, and were encouraged by patronage to withdraw, from active civic participation to the realms of contemplation. They sought an enduring reality, a higher reality than the world of decline around them.

Repeatedly over the centuries, this interpretation of Plato and the ideal-forms of mathematics has been resurrected to deal with similar circumstances. In the twentieth century Werner Heisenberg saw as decisive, in the formation of his own outlook, the experience of reading the *Timaeus* of Plato while hiding on rooftops to dodge the political turbulence and flying bullets of Weimar Germany. That is not the entire story, of course. The return to Plato was part of a recapture of classical antiquity. After a time of vastly different living conditions, products of that older urban lay civilization made sense and seemed contemporary for the first time in a millennium. Authors from the feudal and early medieval periods were the ones who now seemed alien.

The emphasis on systematization is also part of this effort to impose order on a decaying world. The economic slump resulted in extensive rationalization – in Weber's sense – in business; and party factionalism encouraged the growth of bureaucracy to provide some continuity. In another manifestation, competing crafts, to legitimate their claims to higher status, began to produce textbooks, systematic treatments of all knowledge in their fields. This was the next step after the assimilation of classical knowledge.

Copernicus's life and setting mirrored many of these developments. He came from an area south of the Baltic, along the Vistula river. Two centuries before, this had been a land of vast possibilities. His father had been a high member of the merchant patriciate, the rulers of Torun, now increasingly cut off from trade by the decline of the Teutonic knights and by advancing English and Dutch trade routes. This was not a new bourgeoisie but an old monopolist merchant class. As an individual, there were openings for Copernicus, but around him a declining society and class.

These biographical elements do not suffice to explain Copernicus. They enhanced his receptivity to some of the intellectual currents he encountered in North Italy and furnished much of what he was responding to as his own general outlook developed. Not everyone from similar backgrounds, however, was attuned to the new world-view; and the new world-view, of course was far from monolithic. It is much easier to see the underlying direction of a society, to do the social analysis that I have sketched, in retrospect. Sensitivity to incipient developments within one's own society, whether in the sixteenth century or the present, is more difficult. Yet this is the context of creativity and what enables people to take responsibility for their own direction.

You are not bound by the dominant thought-patterns around you. It is possible, and necessary, to see underlying trends – even if they represent potentiality rather than a definitive future outcome. More important still, there is not a one-to-one relationship between social conditions and consciousness, consciousness does not follow automatically from a class or group's specific social existence. Different world-views represent not only different sets of experience, but struggles over different interests. With a clash of world-views, with people freed from taking traditional or dominant thought-

patterns for granted, many possibilities are opened, not simply those based on the old society and those based on some deterministic view of the future. It has been said about the seventeenth century that the struggle in England between emerging groups and those formerly dominant opened up the entire range of theory and political possibilities for the next three hundred years of capitalist society. Many of those possibilities – remember the Diggers? – were bypassed and have remained only as ideals or been forgotten. In its clash of basic perspectives and range of possible alternative directions, Copernicus's time period was somewhat comparable.

It was even more comparable, perhaps, to our own society. Simply as a test case for our questions in social theory, many other historical periods or developments in science would serve equally well. As a parallel to our own complex period of decline with its large range of possible futures, a look at Copernicus's society is instructive. Understanding how the underlying factors interrelate gives us a chance to shape the possibilities.

Copernicanism Beyond Copernicus

The world-view which had originated in a different society no longer made sense, to Copernicus and some of his contemporaries, of their experiences and surroundings. What before had seemed like evidence for a particular theory now seemed irrelevant or absurd. To state this as a more general rule: there are two components in verifying any science theory. One is the correspondence test: that results of an experiment must seem to correspond to a prediction. The necessary qualifications and subtleties are vast. However, the point is that there are severe restrictions to scientists imposing just any interpretation on their sense-impressions. The other component, a criterion of which scientists are usually quite unaware, is what makes sense in terms of the scientists' social existence.

We can see this latter component to some extent by examining the basis on which people responded to Copernicus. Science, of course, consists of the prosaic work of lots of people. The great creators of science, or the people who stand as symbols for the decisive turning points, are but brief moments in the overall flow. And yet those instances of major change in concepts are the best testing

Title page of Galileo's *Dialogue Concerning the Two Principle Systems of the Universe*, 1635 Latin edition. On the left is Aristotle, in the centre Ptolemy and on the right Copernicus. This edition used an authentic likeness of Copernicus, as distinct from the 1632 Italian first edition, which portrayed him as a bearded ancient. Note the loading cranes and commercial ship in the background.

grounds for more general propositions about transforming consciousness. The Copernican Revolution is an especially good example, because none of the criteria which scientists usually cite as determining the outcome of science debates is even relevant. There were no 'strictly scientific' factors, no wholly internal reasons. Copernicus worked from the same data that had been available for centuries. His astronomy is no more accurate than the Ptolemaic astronomy. There was no new technology or new discovery. The telescope did not come until long afterwards. His astronomy is no simpler than Ptolemy's. Contrary to some textbooks, he did not employ fewer circles than some of the Ptolemaic versions then in use. And as we have seen, calculations are more complex using Copernicus's approach.

Over the next decades, the people who became Copernicans did so because of an attraction to the metaphysics. The new premises seemed natural to them, the old obscure and archaic. For some of them, such as Kepler, this was an explicit awareness. For Kepler, Galileo and Newton, even their metaphors testify to their changed society and new world-view, for instance with respect to hierarchy: Kepler searched for a *vis motrix*, a moving physical force patterned after the magnet, a terrestrial mechanism to explain celestial phenomena. Galileo raised the mundane to what had been sublime, comparing the Moon with an ordinary wall in treating the reflection of light from uneven surfaces. And for Newton there was the mundaneness of the sublime: the Sun was like a cookstove in its principle of heat generation.

Most technical astronomers for most of the next century did not convert to Copernicanism. They retained the distinction between cosmology and astronomy, using Copernicus as a mathematical astronomer, while rejecting the world-view. Later in the century, Tycho Brahe developed a system which drew on Copernicus's mathematics, yet held the Earth stationary at the centre of the universe. For a considerable while, most technical astronomers accepted this approach. Yet even this conservative response exhibited the changing premises. Tycho *saw* comets and saw the necessity of day by day planetary observations. He also eliminated the crystalline shells – a step which followed from the Copernican argument, but which Copernicus himself had not taken.

The later Copernicanism, of Newton and beyond, was quite a different matter. It makes a fascinating case study all by itself. These astronomers selectively took elements from Copernicus that made sense to them. They were parallel to Copernicus only in having the Earth move around the Sun, and in the systemic character of their astronomy. Most of the components were quite different. They devalued everything which from our perspective did not aid in human domination of nature. Final causes, aesthetic values, sense qualities; these were still in Copernicus but were eliminated by his later followers. For them, the phenomenon of change was not only accepted, it was welcomed. For a while they had a sense of knowing, or being close to knowing, everything important to know about natural phenomena – manifesting in their science some of the confident outlook of a new class coming to power and flexing its muscles. Early Copernicanism paved the way, but is as far removed from Newton's astronomy as the sixteenth century late Italian and Central European Renaissance is from the late seventeenth century English society after the structural changes and new social alliances of the Civil War.

Science is often presented as the story of great men. If we have focused to some extent on the work of a man named Copernicus, implicit in our discussion have been the grounds for rejecting the 'great man' theory of science. Recognizing the social roots of 'discovery' emphasizes the importance of looking at the acceptance of ideas and not simply at their origin. (People before Copernicus, for instance Aristarchus in the third century B.C., had similar ideas but found their societies unresponsive.) That recognition modifies the usual notion of genius by calling attention to other people in the same field and in quite different fields who were working along parallel lines at the same time; that is, they were using the same new sets of premises and undertaking the same reconceptualization. It also alerts us to the marginal strands of science, the elements that found some acceptance at the time but were bypassed by later generations. To have developed all these points adequately would have required vastly more space. A final implication, however, is worth stressing here as well as later: the advantages for science that would exist in a better constituted society, in being able to draw on the experiences of people hitherto excluded by their sex or class from the practice of science.

Science is also often considered as if it were a different realm of thought, not subject to the same shaping factors as other realms. In fact it is that mythology that prompted me to use it as a test case in exploring the relationship between social conditions and consciousness. What I have really done here, of course, is primarily to focus on one aspect of that relationship: the way in which social conditions influence consciousness. In my concluding remarks I shall attempt briefly to redress the balance and comment on the reciprocal impact of consciousness on social conditions, and on the significance of that reciprocity for a genuinely radical politics. Meanwhile, even within the limits of what I have attempted, there are gaps. For instance, astronomers and cosmologists did not respond to the new theories in isolation as scientists, communing only with surrounding large-scale social structures. There were active efforts to persuade them by proponents of all the different theories. The kind of structures and institutions into which scientists were organized, whether they worked in universities, whether they had professional associations, whether their labs were hierarchically arranged; these are also relevant and interesting factors. They, as well as the politicking among scientists to change concepts, are a story to be told at a different time.

Revolutionary Reconceptualization

In embodying social premises, concepts in science – as in literature, religion, the graphic arts or any other sphere of consciousness – reinforce and legitimate a social perspective. By using the concepts in any field, we also are affirming the hitherto limited sense of human capability and of social possibilities that underlies them. To put it differently, the social experience in a genuinely democratic society – with an enhanced sense of people's ability to work well together, to take responsibility over their own labour and to arrive at policy decisions in the general interest – would give rise to wholly new ways of conceptualizing the problems and received knowledge in all fields. Concepts in science, as in other fields, thus are inherently political.

Every society rests on the consciousness of its members. The major obstacle we face in radical political work is the widespread

conviction that no better society is possible or likely. This cynicism, apathy, low self-regard and lack of imagination – or rather lack of realism, because the potential for a very much better society is quite real – is reinforced by daily experience and the concepts that derive from and are used to order that experience. Radical political practice can build on the understanding of the relationship between consciousness and social conditions developed in our case study.

Knowing that concepts are socially contingent enables a challenge to the society that gives rise to them. We can learn and teach others to identify the hidden, seemingly commonsensical premises that underlie accepted concepts and learn to recognize how these premises reflect a world-view which is socially based and socially restricted. Showing their connection to the structure of our society, understanding all ideas and cultural products in social terms, aids the recognition that this society is not eternal and cannot simply be accepted as a given.

There are limits, however, to working politically with people at the level of consciousness alone. As a large part of this essay has attempted to show, consciousness is not autonomous. There are severe limits on the extent to which people's sense of social alternatives and of their own capabilities to help shape those alternatives can be altered without some alteration in their lives. A revolutionary movement, aiming at a society which will be run by all the people, must engage people increasingly in conscious and active participation towards changing social conditions now. The very fact of being part of political struggle is itself a changed social condition which makes possible changes in perspective and attitude, especially if the political struggle is well chosen and well organized.

If consciousness is mediated in its relation to social conditions in the manner I have sketched, then the goal of radical political organizing must be to change people's overall perspectives. The change required is not at the level of slogans but at the level of world-view how people see what kind of society is possible, how they see their own capability to understand and to join with others to effect change. Through their struggles, people must learn to understand our society, what maintains it and what will be required to change it. There are important answers that will elude us until we have a mass movement with the capacity to shake and test the so-

ciety. People must see themselves as capable, if united, of effecting basic change and increasingly able themselves to decide which tactics will further our growing knowledge and ability to transform. More than an abstract idea of the kind of egalitarian, genuinely democratic society towards which we aim is necessary. Increasingly, our premises must embody that new society as a real prospect and legitimate objective.

Notes

1. As an aside, many of you undoubtedly have a tendency to hear more than what I say. There indeed had been social changes which resulted in people of certain backgrounds thinking of themselves less by their family or town designations and more as individuals. 'The rise of the individual' in its usual treatment, however, carries with it connotations which I want to avoid: both a sense of inevitability about the ultimate growth of capitalism and an implicit propaganda for the merits of bourgeois society.
2. Against the backdrop of the stars, planets occasionally, over the course of a few days, appear to be moving in the opposite direction from their usual flow. This was a puzzle to the old cosmology and one of the reasons why they were called 'planets' – from the Greek word for 'wanderer'. If both they and the Earth were circling some other point, the perceived retrogression becomes understandable, as well as the brightness. Retrogression would occur when their orbit brought them closest to the Earth. You can readily visualize this by moving two hands in front of you in the shape of concentric circles against a stationary background and projecting the apparent motion of each from the perspective of the other.
3. I have argued that the reality/appearance distinction is crucial for Copernican astronomy, and that it appears implicitly and explicitly in other spheres of thought in the same historical period. When I first submitted this manuscript to the *Radical Science Journal* a few years ago, the editorial collective was in the midst of defining its own perspective *vis-à-vis* orthodox Marxism; it was concerned lest I conflate the scientificity of Copernicus with that of vulgar Marxism and thus project back a more contemporary concern into the sixteenth century. In that context a collective member, Les Levidow, wrote to me as follows:

> According to the received version of Marxism, ideology lies in the false, illusory appearances of things, which distort (as a visual metaphor) the underlying material reality of things. From the standpoint of our own Marxism, that version lies within the essence/appearance problematic of philosophy (epistemology, ontology), and has neither agitational usefulness nor grounding in Marx's writings. On the contrary, the only validity to such a distinction is between the social relations on the one hand, and the mystified (reified) form they take on the other hand – not two separate things, not *mere* appearance versus material reality, but a dialectical relation between form and content.

I am in agreement with his comment and consider his own work, for instance on the

IQ debate (*RSJ* 6/7), an outstanding example of the fruitfulness of the approach *RSJ* and others have been developing (from Lukács and others before). Nevertheless, the reality/appearance distinction is not specific to vulgar Marxism, nor does it appear only with bourgeois society. It was known to the Greeks, was new again to Copernicus's time, and is one of the formal structures of argumentation that have recurred when people are in rebellion against a prevailing 'common sense'. Its specific content, its application, what people mean by the underlying reality, vary, of course, from one historical setting to another.

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SOCIALIZING DARWINISM: Historiography and the Fortunes of a Phrase

Jim Moore

> 'I believe in the survival of the fittest and I'm an unfeeling bastard. Darwin does, and he's an evolutionary genius.'
>
> – Precinct Commander complaining to Captain Furillo, 'Hill Street Blues', Channel 4 TV, 2 February 1985

'Social Darwinism' is a pejorative tag. There are more self-confessed fascists and fundamentalists in the world than there are 'Social Darwinists'. Like racists, they do not label themselves; their opponents do. This does not mean that Social Darwinism exists only in the minds of its opponents, any more than racism does. What it means is that those who oppose Social Darwinism need to understand the phrase and to use it critically, if at all. 'Social Darwinism' is a historical artifact – both in the sense that its usage has a history and, more importantly, because the phrase itself depends entirely upon that history for its effectiveness as a polemical device. At a time when the phrase is routinely deployed as a slogan by the Left, and as a scapegoat by those who defend the ideological purity of science, radicals might care to reflect on the *historiography* of 'Social Darwinism' (La Vergata, 1985).

Historiography – the history of historical writing and its assumptions – is worth taking seriously. Those who accept that all human activities are fundamentally historical in character should not need to be reminded that the endeavour to de-historicize knowledge and its production, including the knowledge of nature, or 'science', itself has a history that needs to be unmasked – namely, the history of bourgeois perception. And yet science, or 'the scientific method', is too often regarded on the Left, by a subtle exercise in circular reasoning, as the guarantor of trans-historical objectivity, the very

basis for distinctions between natural knowledge on the one hand and its ideological perversions on the other. Thus Darwinism, it is sometimes assumed, may be so theoretically refined as to serve as a foil for exposing 'Social Darwinism'. The truth of evolution by natural selection as formulated by Charles Darwin in the mid-nineteenth century, and by those who invoke his name, is supposed to have no legitimate affinities with the theory's false historical applications to the social and political relations of humankind.

The aim of this essay is to undermine that distinction, so characteristic of bourgeois philosophy of science, so seductive on the Left. My method is to show historically how Darwinism can be socialized. I shall argue two main points: first, that Darwinism was 'social' from the start; and second, that the problem of 'Social Darwinism' for radicals is, in consequence, not to expose its distortion of true Darwinism but rather to trace both the rhetorical and the conceptual history of the phrase as an artifact of a professional discourse that increasingly pretended to divorce science and ideology.

Two Histories

Within the last ten years, and more especially since 1979-80, when the Thatcher-Reagan alliance was forged, Social Darwinist name-calling has been big news. The liberal *New York Times* has disparaged Social Darwinists on the theatre page (David Mamet's play *American Buffalo*, 5 June 1981), in features ('Solidarism, an Answer to Reagan Darwinism', 2 July 1981), in editorials ('Choate, Darwin, Scrooge & Meese', 17 May 1982), and in the *Sunday Book Review* ('...this is not a book for Social Darwinists or believers in deregulation...', 23 December 1984). Letter-writers complain of 'Financial Darwinism' (7 May 1981) and 'medical Darwinism' in America (31 August 1983), while elsewhere transatlantic commentators debate the signs of the *Times*. Jack Newfield in the *Village Voice* (8 March 1983) opines that the recent 'mood of social Darwinism...was a brief fever that has already broken'. But *The Economist* (20 October 1984), reporting on the upsurge of American libertarianism, declares, 'Social Darwinism...is on the rampage'.[1]

Now conventional wisdom has it that Social Darwinism was the

homespun philosophy of robber barons and other cowboys who rode roughshod over America a century ago. On the frontier and in the marketplace, among men and races and even nations, brutal competition had become the norm. Darwin confirmed the everyday experience of bourgeois individualists that the mighty, not the meek, inherit the earth. The struggle for existence had brought about the evolution of man; now it seemed a bargain price for progress. All would go well, and better still by far, if only well-meaning reformers and reforming governments would let nature get on with the job. The fittest men and races and nations would survive, on the frontier, in the marketplace, round the globe, if left to their own devices. Social Darwinism was thus, according to conventional wisdom, the characteristic ideology of social and political conservatism in late nineteenth-century America.

Small wonder that 'Social Darwinism' enjoys a special status in the vocabulary of American liberalism, or that latter-day cowboys such as Reagan get a bad press as Social Darwinists. But there is more. In the nineteenth century, again it is said, American Social Darwinists looked to biology as interpreted by the English philosopher Herbert Spencer to justify their practices. Spencer had transformed the static moral universe of middle-class Nonconformity into a cosmos of inevitable material and moral progress through the wholesale application of organic analogies and the Lamarckian doctrine of the inheritance of acquired characters to social phenomena. He coined the phrase 'survival of the fittest' after it became apparent that the Darwinian struggle for existence could become an adjunct of his system (Peel, 1971; Wiltshire, 1978; Moore, 1985b). A century later, with Darwin the be-all and end-all of evolutionary science, it is not hard to see why Spencer, too, gets a bad press as a Social Darwinist, at least among biologists and historians.

But today those who are called Social Darwinists in America need not rely on Spencer. They may, if they like, draw support from their own biologists – 'sociobiologists'. A decade ago E.O. Wilson of Harvard University, who reportedly looks upon Spencer as 'one of his heroes' (Ruse, 1986b, p.97), proffered sociobiology as a 'new synthesis' in a manner redolent of Spencer's massive *Synthetic Philosophy* (1860-96). According to Wilson, sociobiology is 'the systematic study of the biological basis of all social behavior'. Needless to

say, however, no self-respecting sociobiologist would claim to be a conservative Social Darwinist like Spencer. Wilson himself states in *On Human Nature* (1979, p.215) that the world as envisioned by Spencer's nineteenth-century disciple, 'the absolute Social Darwinist' William Graham Sumner, is 'biologically impossible'. David Barash claims in *Sociobiology: The Whisperings Within* (1981, pp.237-8) that Social Darwinism is a 'misuse' of evolutionary biology and 'has now been totally discredited'. Richard Alexander rejects 'any notion' that his arguments in *Darwinism and Human Affairs* (1980, p.233) lead 'to an acceptance or promotion of a social Darwinist future'.

Their opponents disagree. No conscientious critic of sociobiology, imbued with the conventional wisdom about Social Darwinism as the ideology of social and political conservatism, will scruple to apply the phrase to the most characteristic evolutionary doctrine of Reagan's America. In fact the deed was done as soon as the doctrine emerged, before Hollywood came to the White House. In Britain the BSSRS Science as Ideology Group, writing in *New Scientist* for 13 May 1976, were quick to call sociobiology 'the modern successor to Spencer's Social Darwinism'. 'Its basic theoretical position', countered Steven Rose in the next issue, 'is not scientific but ideological, and an ideology which by "justifying" a present, unjust social order, denies the possibility of change and proclaims its righteousness just as surely as did Social Darwinism in Victorian England.' In America Stephen Chorover (1979) made the same historical connection by referring to 'Social Darwinists' from Spencer onwards as 'sociobiologists', while numerous other opponents, especially members of Science for the People, have since embellished this familiar Yankee theme.

Left opponents of sociobiology correctly evoke the historical and political contexts of scientific debate in order to expose the tendencies of the 'new synthesis'. In these contexts 'Social Darwinism' has been attached not only to American conservatism, but to a sordid succession of other capitalist, racist, sexist, imperialist, and militarist ideologies. Accordingly, on the Left it remains a term of reproach. Liberal critics of sociobiology have, in turn, found it 'ironic' that politically motivated people – Left scientists in particular – should accuse others of being politically motivated as well

(Himmelfarb, 1981, pp.459-60). But in fact liberals are just as apt to cry 'Social Darwinism!' when their historical and political interests are threatened. For instance, Mary Midgley has mercilessly dissected Britain's own home-grown sociobiologist, Richard Dawkins, who claims in *The Selfish Gene* (1976, p.210) to represent the 'essence of Darwinism' rather than its deterministic social consequences. In an essay entitled 'Selfish Genes and Social Darwinism' (1983, p.366) she argues that 'sociobiological thinking, especially in its Dawkinsian form, actually reinforces Social Darwinism, both by its language and by some of its substance'.

Why did Midgley, a professional philosopher and *Guardian* columnist, become 'indignant' with Dawkins's work? What made her overcome a former reticence, as she says, 'to break a butterfly upon a wheel'? (Midgley, 1979, p.458). The answer reveals a second reason why 'Social Darwinism' is a term of reproach. Midgley and Dawkins agree, she argues, that 'Darwin's theory of natural selection is...much the best guide we have' to understanding the human constitution. This being so, the theory must be protected from 'that early, widespread and deep-rooted bunch of misunderstandings of Darwin's ideas, which is called (somewhat misleadingly) Social Darwinism'. But, according to Midgley, Dawkins has let down the side by using loose language to express flabby concepts. He has been culpably careless with a theoretical tradition; he has impugned the reputation of its founder. And Darwin must at all costs be absolved of the excesses of those who made illegitimate 'applications' of his theory. 'Radical or liberal' critics of sociobiology, whose 'real objection is political and moral', must be made to see that their 'real opponent is still Spencer'. Midgley accordingly dedicates her latest book, *Evolution as a Religion* (1985), 'To the Memory of Charles Darwin Who Did Not Say These Things'.

It appears, then, that 'Social Darwinism' is both a label for one's enemies and a peril for one's friends. Social Darwinists keep historical bad company, on the one hand; on the other, they fail to keep faith with the past. They *betray* a history in both senses of the word. One is a history of *bad precursors*, which Social Darwinists are held to disclose. The other is a history of *good ancestors*, which Social Darwinists are said to disgrace. These two histories go together like the positive and negative of a photograph. The image of Social

Darwinism they convey will bear closer scrutiny than it has previously received. It may be not only that 'Social Darwinism' depends on faulty historical judgements for its effectiveness as a polemical device, but that the Left's understanding of the phrase is in serious need of revision.

History I: Good Ancestors

Spare a thought for the poor sociobiologists: attacked by the Left, rebuked by the liberal Right, deprived of Darwin as a precursor. At least Darwin's example remains to console them, for Darwin has been attacked as a Social Darwinist as well. And everyone, from Midgley on down, regards this as wrongheaded if not perverse. Only Darwin's critics think he was a Social Darwinist, and they are political gulls.

Now, at the the best of times it is dangerous enough to tamper with Darwin's reputation. But to suggest, in the face of the creationist threat, that the great naturalist may have been as politically inspired as oneself is to invite public abuse. 'Burning Darwin to Save Marx' runs the title of Tom Bethell's snide critique (1978) of Left biologists in *Harper's* magazine. The accusation is picked up in the last chapter of Yves Christen's very clever polemic, *Marx et Darwin: Le Grand Affrontement* (1981). 'Quelles qu'aient été ses convictions', he concludes comfortably from the editorial chair of *La Recherche*, 'Darwin resta discret sur la question du darwinisme social.' Only Marxists and other religious fanatics would have it otherwise – a view strongly endorsed if not actually asserted by Antony Flew in his rather less clever and more polemical *Darwinian Evolution* (1984a; cf.Flew, 1984b).

Nor is this assessment of Darwin peculiar to biologists and their journalistic poodles. Popular historians may not pillory their opponents, but they routinely follow consensus and exculpate Darwin from any responsibility for Social Darwinism. James Joll, for example, in his standard Penguin text, *Europe since 1870* (1976, pp.102-3) states coyly that Darwin 'had not himself devoted much attention to any social implications his ideas might have had'. But these ideas 'were rapidly applied to questions far removed from the immediate scientific ones with which Darwin himself had been con-

cerned', and thus they 'inspired much of which the author might well not have approved'.

Among historians of science the quest to cleanse Darwin from any taint of ulterior ideology has remained a positive preoccupation, notwithstanding the poetic justice of Sir Gavin De Beer's discovery some twenty years ago of conclusive manuscript evidence linking Darwin with Malthus. I well remember my own tortuous attempt within the recent past to launder Darwin's language (Moore, 1979, pp. 160-1). Unsurprisingly, it took two outsiders to Darwin scholarship to see what was going on. Posing as anthropological observers of the field – they are in fact historical sociologists – Steven Shapin and Barry Barnes (1979, p.127) suggest that Darwin's defenders are in reality trying to protect the sacred from pollution with the profane.

> The defence...has rested upon three assertions. The first is that of internal purity: Darwin's *intentions* and *motives* in writing the *Origin* were above reproach, and his personal *beliefs* in 1859 were innocent of 'ideological' taint. The second is purity of ancestry: 'influences' upon the *Origin* were entirely wholesome and reputable; in particular, nothing 'ideological' was gleaned from Malthus. The third assertion is purity of germ-plasm: nothing untoward could *properly* be deduced from the theory in the *Origin*; truth does not blend with error; insofar as truth was used to justify social Darwinism, it was misused.

However, on all three counts the defence collapses, as I hope to show.

What is significant about this analysis for the present is that it does not mention the *Descent of Man*. Here if anywhere, one would suppose, Darwin must stand trial for Social Darwinism. Here if anywhere his acolytes must guard against ideological slips twixt cup and lip. The long neglect of Darwin's major statement on human evolution, by both scientists (except Campbell, 1972) and historians (except Jones, 1978), is indeed an eloquent silence, but since Shapin and Barnes published their little field-study the silence has twice been broken resoundingly by Darwin's defenders. Sir Andrew Huxley, President of the Royal Society and grandson of T.H., addressed the question posed in the title of his 1982 centenary lecture, 'How far will Darwin take us?' (1983) by stating that Darwin

'kept off mental phenomena' in the *Descent of Man*. 'Darwinism' as expressed in the book was concerned only with 'bodily features' (pp.14, 16). Since Darwin devoted between three and four chapters of the *Descent of Man* to 'mental powers', 'intellectual and moral faculties', and the like, we may pass immediately to a historian who makes closer distinctions. Yvette Conry, whose *L'introduction du darwinisme en France au XIX^e siècle* (1974) is the most impressive if not the least problematic historical account of post-Darwinian developments yet to appear, also took up the *Descent of Man* in the Darwin centenary year. She had satisfied herself in *L'introduction* by 'une récurrence à partir du XX^e siècle' (p.425), that Darwinism is pure science, unadulterated by ideology. From which it followed that Darwin's racial theorizing and doctrine of progress in the *Descent of Man* reveals a 'darwinisme idéologique', and the book itself thus becomes 'un ouvrage déviationniste', or 'peut-être...un texte pré-darwinien' (Conry, 1983b, pp.177, 178).

To refer to anything Darwin wrote as 'pre-Darwinian' strikes me as confusion confounded – unless, after all, Darwinism is a Platonic essence beyond the reach of mere historical contingency. I expect even the most ardent defenders of the theory of natural selection in its pristine purity, including certainly all the biologists, would find the suggestion counterintuitive. They would prefer to follow Huxley in misreading or misrepresenting inconvenient portions of the *Descent of Man*, rather than contrive, as Conry has done elsewhere (1983a) with real skill, to demonstrate how the great Darwin, enticed by Spencer and misled by his own insights on the 'Beagle', was diverted from applying the scientific theory contained in the first edition of the *Origin of Species* into constructing specious biological arguments for Social Darwinist ideology. ('*La Descendance* n'est pas la suite de l'*Origin des espèces*, mais sa chute' – p.36.) The fact, however, that two scholars, both eminent in their own right, are prepared to countenance such interpretations suggests that something very important is at stake. It will pay to look again at the *Descent of Man* to discover what it is. Perhaps sociobiologists may take more than comfort from Darwin's example after all.

Darwin's 'Social Darwinism'

Most people know that Darwin did not invent the idea of social evolution. The illusion that he did persists only because a succession of well-known writers – Maine, Spencer, Lubbock, McLennan, Tylor – published works on social evolution soon after the *Origin of Species* appeared. *Post hoc, propter hoc*, the saying goes. But we now know, thanks to John Burrow's *Evolution and Society* (1966), that these writers derived little from Darwin. Their theories were largely a response to the earlier failure of philosophical radicalism and positivism to found a science of society on a static conception of human nature in the face of a collapsing Christian cosmology and an industrializing economy. Indeed, Darwin himself confronted this impasse; and I have argued elsewhere (Moore, 1982b) that his work, like that of Spencer, his younger contemporary, must be seen as one of a range of related attempts by bourgeois intellectuals after about 1840 to naturalize morals and domesticate social conflict in the interests of progressive reform. On this view the so-called Victorian 'crisis of faith' becomes a crisis of legitimation; the so-called 'Darwinian Revolution' appears as the ideological counterpart of a wider *resolution* of social forces in which Darwin came to play a powerfully symbolic role (Moore, 1982a, 1986, 1987).

And on this view Darwin himself becomes an authentic creature of his times, ransacking current literature for explanations of the world as perceived by a mid-Victorian man-of-means. He read the political economists and accepted their version of how the world works. He read of Comte and discerned that history is governed by law. Above all he read the Reverend Thomas Malthus and found the law he needed to unify nature and history: human populations naturally tend to outstrip their food supply, as a geometric series does an arithmetic; this arrangement, resulting in starvation, disease, poverty, and a struggle for existence, has been divinely ordained to promote, through sexual restraint, the material and moral improvement of the human race (Turner, 1986).

Since the day in 1832 when Darwin first set English eyes on native Fuegians and wrote, 'The difference between savage & civilized man...is greater than between a wild & domesticated animal, in as much as in man there is greater power of improvement' (Darwin,

1933, p.119), his growing conviction that species were mutable became inseperable from his belief in the evolution of humankind. It followed that generalizations about human beings, such as Malthus's 'principle of population', could be applied directly to other species; generalizations about these other species, in turn, would be found to hold true for society. Darwin kept up this interchange in his private notebooks with the express intention of providing a full-blown naturalistic and materialistic explanation for all the phenomena of human life – body, mind, instinct, behaviour, morals, and religion. No sociobiologist, not even the Pulitzer Prize-winning author of *On Human Nature*, E.O. Wilson, has conceived a more audacious metaphysical research programme than Darwin had privately undertaken by the time he was thirty years old.

The *Descent of Man* in 1871 was one belated spin-off from this programme. It was not, however, created suddenly *ex nihilo*. Darwin drew extensively on the notes he had made thirty years earlier; the continuity of his views on human nature is reminiscent of his stable domestic existence over the same period (Moore, 1985a; Richards, 1983). And he embellished and augmented these materials from the writings of social evolutionists who had come to prominence in the 1860s. Burrow, in the second edition of *Evolution and Society* (1970, pp.xix–xx), admits that his denial of a major Darwinian 'influence' on the social evolutionists may seem trivially true, for he omitted to consider individuals such as Walter Bagehot who were influenced by Darwin. 'In retrospect...my reasons for neglecting Social Darwinism look suspiciously Whiggish.' This does appear to be the case, but what Burrow might equally have said is that his unproblematic concept of Social Darwinism had led him to neglect the influence of social evolutionists on the *Descent of Man*, including those who are regarded as Social Darwinists. For the book and its author were 'pre-Darwinian' in this sense alone: their intellectual pedigree was not 'good' by the standards of Darwin's defenders today.

It is often forgotten that, before the *Descent of Man* appeared, commentators had a dozen years to draw out the implications of the *Origin of Species* for human beings. And they did so prolifically. Darwin, who was well read, had every opportunity in the interval to work their views into his previous ideas of social evolution. In the

Descent of Man he frequently cited social evolutionists – Maine, Lubbock, McLennan, Tylor – whose views were developed more or less independently of his own. However, as John Greene has shown in a crucial article ('Darwin as a Social Evolutionist', in Greene, 1981), he also drew heavily on so-called Social Darwinists.[2] From Spencer he obtained (via Wallace) the notion of racial superiority as dependent on 'intellectual and moral' qualities; from Francis Galton, the doctrines of 'hereditary genius' and what Galton later called 'eugenics'; from W.R. Greg, an arch-Malthusian, the problem of communal checks to the free play of natural selection; and from Bagehot, various insights related to the contingency of progress on group cohesion, the consequences of racial mixing, and the effects of selection by war and military conquest. Greene concludes 'that what we call "social Darwinism"...was endemic in much of British thought in the mid-nineteenth century, that Darwin's *Origin of Species* gave a powerful boost to this kind of thinking, and that Darwin himself was deeply influenced by this current of thought' (p.123). This is neither a radical nor a materialist formulation (cf.Williams, 1974), but for that reason Darwin's defenders have probably taken it more seriously.

Before seeing how seriously Darwin's defenders have taken Greene's analysis, it is worthwhile to be reminded just how much of a Social Darwinist Darwin was. Anyone can do this in an hour's spare time.[3] Take a central extract from one of the chapters of the *Descent of Man* that, on Sir A. Huxley's view, should not exist, and add to it the relevant portion of Darwin's 'General Summary' at the end of the book (Darwin, 1874, pp.133–43, 617–18). Then underline all the terms Darwin employs to describe those whom he regards as the fittest, on the one hand, and on the other, as the less fitted to survive the struggle for existence. When I did this exercise I made up lists of terms grouped roughly in accordance with Darwin's references to physical, intellectual, moral, and ethnic or racial characteristics.

the fittest	*the less fitted*
'the able in body and mind'	'the weak in body or mind'
'the sound'	'the imbecile'

the fittest	*the less fitted*
'the finest young men'	'the maimed'
'civilized men'	'the sick'
	'shorter and feebler men, with poor consitutions'
	'parents who are short-lived'
	'savages'
'the intellectually superior'	'the inferior'
'civilized races'	'lower races'
'the better class of men'	'the reckless and improvident'
'the careful and frugal, who are generally otherwise virtuous'	'the vicious and otherwise inferior members of society'
'the prudent'	'malefactors'
'the frugal, foreseeing, self-respecting, ambitious Scot'	'melancholic and insane persons'
	'violent and quarrelsome men'
	'the restless who will not follow steady occupation'
	'the intemperate'
	'profligate women'
	'the careless, squalid, unaspiring Irishman'
	'the poorest classes'
'[the race of] Saxons'	'[the race of] Celts'
'the English as colonists'	'other European nations'
'Canadians of English... extraction'	'Canadians of...French extraction'
'Anglo-Saxon people'	

Key words among these terms, such as 'frugal', 'virtuous', 'prudent', and 'improvident', indicate a Malthusian mode of discourse. Malthus, whose *Essay on the Principle of Population* (1798) reached a seventh edition before his death in 1834, provided a 'common context' for discussion of biological and social theories throughout the nineteenth century. His work had a formative influence on most of the leading Victorian evolutionists (Young, 1969; reprinted in Young, 1985b). Spencer, Wallace, Galton and Greg were all devout Malthusians, and the extracts from which the terms above are taken contain Darwin's synthesis of their views with his own. Certain 'checks' to population growth – another Malthusian key-word – are

mentioned repeatedly in the extracts, and it is straightforward to classify these references as follows:

checks ensuring the survival of the fittest	*checks inhibiting the survival of the fittest*
inability of the weak and helpless to marry	care of the 'weak and helpless'
failure of inferior people in trades or professions	the 'finest young men' go to war while inferior men marry and reproduce
'civilized races' taking the place of 'lower races'	accumulation or inheritance of immoderate wealth
loss of wealth by the foolish or profligate rich	multiplication of 'the very poor and reckless'
elimination of the worst moral dispositions by death or interference with reproduction	late marriages of the prudent
high mortality among the urban poor	celibacy
high mortality among those who marry too early	

In these extracts Darwin seems generally to have favoured expediting the checks that ensured the survival of the fittest and eliminating, where possible, the checks that inhibited it. Among the latter, the only check he specifically exempted was the first: 'We must... bear the undoubtedly bad effects of the weak surviving and propagating their kind', for to do otherwise would cause a 'deterioration in the noblest part of our nature', which is itself the product of natural selection. Also, since Darwin believed that 'highly civilized nations...do not supplant and exterminate one another', he may have regarded the loss of the 'finest young men' in war as a passing problem. The multiplication of 'the very poor and reckless' could be abolished *à la* Malthus if all refrained from marriage who were 'in any marked degree inferior in body or mind' or who could not 'avoid abject poverty for their children'. Those superior individuals, on the other hand, who practice moral restraint or celibacy are informed: 'There should be open competition for all men; and

the most able should not be prevented by laws or customs from succeeding best and rearing the largest number of offspring.' This in turn would be tantamount to encouraging the failure of inferiors and the winnowing of the wealthy in the marketplace.

Again, like Malthus, Darwin quite accepts, if not encourages, the high rate of mortality among the urban poor and in early marriages. Yet he attaches great importance to the population growth that results in colonization, the elimination of 'lower races', and the expansion of empire.

The Malthusian Legacy

The *Descent of Man* undoubtedly contains a ragbag of insights and equivocations (Conry, 1983b; Alland, 1985). Here, however, in two connected extracts, are all the basic pretexts necessary for 'Social Darwinism' to thrive. Competition and cooperation, reaction and reform, internal and external Social Darwinisms based on individual, class, race, sex, and national rivalries – Darwin mediated the Malthusian common context of language and assumptions for all these biological interpretations of history. He was, as Antonello La Vergata suggests (1985, p.960), 'one of the very few Social Darwinists who was really a Darwinian'.

Commentators on Darwin and Malthus have not read the *Descent of Man* in this way, or, as seems more likely, they have not read it at all. Darwin's debt to Malthus has been documented with ever-increasing precision since De Beer uncovered the crucial manuscript twenty years ago. But attention has been focused almost entirely on the genesis of the theory of natural selection in 1838 and its development until 1859 (Kohn, 1980; Ospovat, 1981). That is to say, the contribution of Malthus to what is now regarded as scientifically 'correct' has been the paramount concern, while Darwin's lifelong Malthusianism and the integral function of the principle of population in the evolutionary mechanisms of the *Descent of Man*, as well as the *Origin of Species*, has been either overlooked or strictly circumscribed (Conry, 1983a).

Instead, Malthus has remained important chiefly for the study of Social Darwinism. One recent writer has tried to show how differently Malthus and certain Social Darwinists interpreted 'la Nature,

l'homme, les formes du lutte pour la vie, le rôle de l'Etat et la notion de progrès' (Béjin, 1984, p.80). Another, Allan Chase, has fingered Malthus as the 'founding father' of 'scientific racism'. In a blockbuster entitled *The Legacy of Malthus* (1980) Chase treats Social Darwinism as 'pseudo-science' – an 'intellectual aberration'. Bouncing from Malthus to Spencer to Galton and into the twentieth century, he explodes racist arguments left and right. In 700 pages Darwin is mentioned exactly twice, to dissociate him from Spencer.

But the legacy of Malthus, including racism, was Darwin's as well. The underlying continuity between the Darwinian and the Malthusian views of nature, human nature and society must remain integral to any historical assessment of Social Darwinism. Both Malthus and Darwin believed in the beneficent necessity of the laws of nature that give rise to a struggle for existence in human populations. Both believed the dictate of these laws was that individuals ought generally to enjoy the fruits of their foresight or suffer the pains of their improvidence. Both believed that the degrees of material success or failure in question are direct indications of moral worth and, as such, ought not to be mitigated.

For example Malthus undertook to abolish arrangements for assisting the poor, which lead to social vice and misery. He encouraged the 'proper check' of moral restraint as conducive to a virtuous and industrious population. Darwin favoured, with certain qualifications, the abolition of arrangements for preserving the unfit, which lead to evolutionary degeneration. He urged moral restraint on the unfit and open competition for the rest in order that some should 'succeed best', rear 'the largest number of offspring', and thereby constitute the 'fittest' who survive. The only 'checks' to the ruthless exploitation of these principles were, in Darwin's case, his humanitarian concern for the 'weak in body or mind' and his touching mid-Victorian belief that 'highly civilized nations...do not supplant and exterminate one another as do savage tribes'. In other times and places these restraints would be readily abandoned.

Late-twentieth-century America may yet prove to be a case in point if sociobiology underwrites a new 'Social Darwinism', as critics fear it will. The likelihood of this occurring on a large scale will not be diminished if sociobiologists succeed in identifying their theoretical conclusions with those of the *Descent of Man* and its re-

vered author. Wilson's *Sociobiology* was published in 1975; Greene's analysis of Darwin's 'Social Darwinism' appeared two years later. Since then a new history has begun to emerge in which Darwin is *not* to be defended from charges of Social Darwinism; on the contrary, sociobiologists are to be defended from their critics on the grounds that *they* alone are Social Darwinists after the manner of Darwin.[4]

In 1980 if not earlier Michael Ruse began to argue (1980b, p.32), using Greene's analysis, that *both* Darwin and Spencer were Social Darwinists, albeit 'of very different kinds'. The qualification is crucial. Darwin, according to Ruse, was a better Malthusian. For him, unlike Spencer, 'there was no question' that the struggle for existence in human populations would ever come to an end. Evolution by natural selection was not optimistic but 'opportunistic'; it could well result in human 'degeneration' if the unfit were preserved. Ruse concludes (p.36) that today we should expect to find the same differences as existed between Darwin and Spencer among those who look to biology 'to justify and illuminate social beliefs'. Among Darwin's 'present-day successors' he nominates E.O. Wilson (see also Richards, 1982).

Ruse has gone on (1980a) to portray Darwin as sociobiology's great progenitor. In 1981 he took the stand in Arkansas against the creationists, speaking as a philosopher of science (Ruse, 1984); in the centennial year he published a popular polemic, *Darwinism Defended: A Guide to the Evolution Controversies* (1982a). Recently the eminent and prolific public figure of Michael Ruse has been at Harvard working with Wilson himself, possibly in a manner that will recall other spokesmen for contentious biological theories. 'I... look at Darwinism to see seeds of ideas we presently find important', he has remarked candidly (1982b), and Wilson now follows suit. Since dissociating himself from Social Darwinism in *On Human Nature*, Wilson has dilated (1983) on 'Darwin's contributions to sociobiology'. Darwin, he says, reached 'remarkably modern conclusions' on the evolution of insect societies (Wilson's specialism); he 'anticipated the modern principle of kin selection leading to altruism'; his 'intuition' about group selection in the *Descent of Man* was 'essentially sound' (Wilson quotes at length from the chapter discussed above); his views on sexual selection have

'proved seminal'(!); and his insight into the main problems of ethology today in *The Expression of the Emotions in Man and Animals* (1872) was 'truly visionary'. From here it appears only a small step to enlist Darwin's reputation, as Ruse has done in *Taking Darwin Seriously* (1986b, pp.255–6, 271), to resist 'wholesale proposals for change' in Western moral attitudes, to question the advisability of relief efforts in the Third World, and to label people without forty-six chromosomes or who lack 'our moral sense' (possibly including other races) as 'abnormal, and (probably) sick'. 'As the twentieth century draws to its close', he observes innocently, 'Social Darwinism (if not by that name) seems to be enjoying a renaissance, if that is the appropriate term for such a notion' (p.79; cf. Ruse, 1986a).[5]

History II: Bad Precursors

The flip side of a good ancestry is tainted blood. The counterpart of defending the pure Darwinian germ-plasm is to attribute blame for Social Darwinism to miscegenation, the improper 'application' of Darwin's theories, the illicit mingling of science and ideology. This requires the allocation of historical scapegoats; and a good deal of the literature on Social Darwinism is taken up with identifying them correctly.

Richard Hofstadter's famous study, *Social Darwinism in American Thought*, originated in a doctoral dissertation completed at Columbia University in the early 1940s. Hofstadter had come to intellectual maturity during the political and moral controversy between liberals and conservatives over the New Deal; the dissertation, published in 1944, was a belated intervention in this debate. His liberal interpretation of Social Darwinism soon became the 'conventional wisdom' summarized at the outset of this essay. Social Darwinism to him was a unitary and more-or-less coherent body of conservative ideas originating in the writings of Herbert Spencer. It was the dominant ideology of the American entrepreneurial class until at least the 1880s, when a host of 'dissenters' led by Lester Frank Ward attempted to 'wrest Darwinism from the social Darwinists' by showing that its 'consequences' could be 'read' in the interests of reform. 'Today', Hofstadter reiterated in the revised edition of his book (1955b, p.6), 'their arguments seem, if not

to everyone superior, at least to most of us an indispensable antidote to the plausible arguments of the men they criticized.'

In all this there was scarcely a hint (pp.77–8) that anyone else could have been a Social Darwinist except the ideological forebears of Hofstadter's mid-twentieth-century conservative opponents. Darwin, needless to say, was not among them. Cooperation had as little to do with Darwin's theories as competition did, reform as little as reaction. 'Darwinism had from the first this dual potentiality; intrinsically it was a neutral instrument, capable of supporting opposing ideologies' (p.201). Only 'scientism' would adulterate Darwin's theories with metaphysics; and in *The Age of Reform* (1955a) Hofstadter made clear that this charge also applied to the liberal opponents of Social Darwinism. Only a liberalism cognizant of history and divorced from metaphysics could proclaim the 'end of ideology'.

Subsequent writers have embellished the liberal view of Darwin's theories, although Hofstadter's interpretation of Social Darwinism was eventually undermined. In *Darwin: Competition and Cooperation* (1952) Ashley Montagu still blamed Spencer for Social Darwinism. He held that Darwin had overstressed competition but that the *Descent of Man* was only 'ambivalent', and Darwin, had he lived, would have sided with Kropotkin and himself. Bert Loewenberg in *Darwinism: Reaction or Reform?* (1957) disagreed. On the whole Darwin had favoured 'mutual aid and cooperation'; Darwinism had 'two main types of social interpretation', one conservative and reactionary, the other devoted to liberal reform.

This analysis marked a new departure. Within a dozen years Hofstadter's unitary conception of Social Darwinism disintegrated. In 1959 Irwin Wyllie removed the typical American businessman from the taint of Spencer's influence; in 1965 David Burton extricated the rough-riding President himself, Theodore Roosevelt, from Darwin's; and by 1968 Gertrude Himmelfarb had picked out at least a half dozen 'varieties of Social Darwinism'. The process still goes on – Sumner now looks like a liberal (Garson and Maidment, 1981); pluralism looks like Darwinism's only politics (Crook, 1981) – and this notwithstanding the sophisticated attempt by R.J. Halliday in 1971 to define Social Darwinism simply as eugenics.

Meanwhile a genuine Right-revisionist was in the making, one

Richard Bannister, who has tried to stand Hofstadter on his head. Beginning with an article in 1970 that pointed out 'the paucity of *bona fide* examples of conservative Darwinism' – Gilded Age 'histrionics' had hitherto been mistaken for history – Bannister worked out the view that Hofstadter's heroes were the real culprits; 'Social Darwinism' was a 'myth'. In *Social Darwinism: Science and Myth in Anglo-American Social Thought* (1979) he argues that liberal reformers were 'from the first *the* Darwinian social theorists' (p.253). These 'activists' (p.11) attacked conservatives as Social Darwinists for supporting individualism, free enterprise, and *laissez-faire*. The label stuck, despite the fact that conservative social theory owed little or nothing to Darwin and everything to 'Enlightenment fundamentals' (p.136). Thus arose the 'myth' of conservative Social Darwinism – a myth abetted by Hofstadter – which has served in turn to conceal the liberals' own indebtedness to Darwin. 'Their theory that human control must replace the laws of nature was', according to Bannister, 'the accurate reading of Darwin's theory' (p.15).

Here, on shaky ground, Bannister is at his most daring. To accomplish his *tour de force* he attempts two major feats of historical skill. First, in order to establish a good lineage for conservatives, he attempts to reconnect Spencer, the battle-scarred author of *The Man versus the State* (1884), with the old 'Enlightenment liberalism' of the *Wealth of Nations* and the Declaration of Independence. This is easy: Spencer abandoned the 'lofty idealism' (p.78) of his *Social Statics* (1851) primarily because of the influence of Darwin, who complicated and ultimately frustrated his time-honoured strategy of 'basing social theory on laws of nature' (p.55). Second, in order to correct those who have 'distorted', 'twisted', and 'misrepresented' Darwin, Bannister attempts to show that 'Darwinism was neither neutral nor inherently conservative' (p.15). This, too, is easy: Darwin disrupted the natural harmony that Spencer and other conservatives ascribed to society, struggling with its environment, by extending the Malthusian struggle to include *both* intra- and interspecies competition. Darwinism opened a 'gap between society and nature'; he undermined the 'intellectual strategy' of social theorists since Newton who believed that 'sound social policy was the result of allowing free play to the automatic operation of natural law'.

Thus he gave rein to reformers who demanded 'increased governmental regulation; new efforts of social welfare and control; and a more positive role for America abroad'. 'In this sense', Bannister declared, 'all social Darwinism *was* reform Darwinism' (pp.11, 24–5, 33, 158). Certainly he would leave no doubt in the mind of Reagan's America that liberals were to blame.

The 'mythical' conservative-Republican interpretation of Social Darwinism is just as much a period piece as Hofstadter's 'unitary' liberal-Democrat interpretation, which suited American society forty years ago. Bannister's sins, however, are less forgivable; his performance fails, not for lack of collateral scholarship, as in Hofstadter's case, but because this has been ignored. Leaving aside the historical question of 'Social Lamarckism' in America (Livingstone, 1985), anyone who finds it 'something of a mystery' (p.24) that Malthus should have influenced Darwin has not read Darwin's published notebooks. Anyone who has not read these notebooks and their interpreters (e.g. Schweber, 1977) will not know how considerably Darwin was influenced by Adam Smith and later exponents of classical liberalism. Had Bannister even read the writings of Michael Ghiselin (1969, 1972, 1974, 1978) he would have found congenial discussions of Darwin as the scientific standard-bearer of competitive individualism and free-market economics.

And can we rightly see Malthusian natural harmony as a wedge between Darwin and Spencer? First, there is no neutral calculus for separating the descriptive from the ideological in Malthus's account of the struggle for existence. Second, Malthus unquestionably held that an *interspecies* struggle had promoted material and moral progress, and would continue to do so (Levin, 1966; LeMahieu, 1979; Santurri, 1982). Third, Darwin's published notebooks, as well as subsequent scholarship (Ospovat, 1979), show clearly that Darwin *incorporated* Malthusian natural harmony into his early evolutionary speculations. Fourth, progress and natural harmony remained persistent underlying themes in Darwin's work, even to some extent until the end of his life (Ospovat, 1981). Finally, as I have shown, the *Descent of Man* mediated Malthusian language and assumptions to a wider audience, where they were labelled 'Social Darwinism'.

Towards a Socialized Darwinism

By 1980 Hofstadter's 'Social Darwinism' had disintegrated; the scholarly search for 'bad precursors' had reached its nadir in Bannister's special pleading. The time was ripe, both politically and historically, for a study such as Greta Jones's *Social Darwinism and English Thought: The Interaction between Biological and Social Theory*. Jones is a committed critic of Social Darwinism; her sympathies lie with the Left. Although her book will frequently frustrate those who share her sympathies, it does analyse most suggestively how and why Social Darwinism has assumed its protean forms.

Jones's interpretation of Social Darwinism is almost unremittingly intellectualist – more so, ironically, than Bannister's and Hofstadter's. 'Currents of thought', 'sets of ideas', 'intellectual atmospheres', and even 'intellectual ideas' ramify through the text. 'Social Darwinism', which appears as a 'unified discrete area of thought', is often hypostatized: it is said to have 'removed', 'substituted', 'reasserted', or 'secularized' one thing or another. Darwin never appears as a Social Darwinist. Jones minimizes his early interest in human development as an inspiration for his work; she contrasts Malthus's conservatism with his belief in progress. What Darwin accomplished, she says, was to give 'a naturalistic explanation for the emergence of an essentially ideological version of contemporary human social and mental evolution' (p.12). He allied an 'important scientific discovery' to a 'certain conglomeration of social and cultural ideas'. But in doing so he, like Wallace, 'lapsed' and 'succumbed' to these ideological assumptions; he was 'trapped' and 'mesmerized' by concepts such as racial hierarchy, progressive development, and savage survivals. Thus in the *Descent of Man* Darwin 'helped begin the creation of Social Darwinism'. Social Darwinism was 'an elaboration by others of what they consider[ed] to be Darwinian interpretations of society' (pp.10, 194).

By what process did this occur? Although Jones keeps science and ideology, Darwinism and Social Darwinism, quite distinct, her answer is potentially illuminating:

> In politics some ideas survive and grow and others do not. There were political and social limits on the 'free market' of ideas. Cultural and

> social forces picked out some among the litany of social Darwinism and neglected others. So that it is possible to identify, even given the wide spectrum of social Darwinist ideas represented in British and American society, a certain political direction which social Darwinism took as far as its general social influence is concerned. (p.ix)

What Jones intends to communicate through these muddled metaphors is, I think, that the *language* in which the 'ideas' of Social Darwinism found expression was appropriated selectively from time to time to confer legitimacy on dominant social interests and political formations, which in turn have given 'Social Darwinism' its historical character.

This is a point worth noting, and I shall return to it in due course. For the present it only needs explication. Jones recognizes that Darwinism itself, considered abstractly as the theory of natural selection, 'shared a vocabulary with social thought' and therefore 'had links with a much broader culture' (p.5). She also acknowledges that 'Darwin's theory of the development of human faculty and society' was at least partly derived from the theory of natural selection, and thus associated with a 'powerful social metaphysic'. So she can see that

> what characterizes much of social Darwinism is the retranslation of its terms [i.e. belonging to Darwin's theorizing in the *Descent of Man*] into those of the natural and moral economy of pre-Darwinian social thought. Along with the development of Darwinian science went a concomitant process in which its terms and vocabulary were incorporated into highly restrictive and ancient social aphorisms. (p.181)

Elsewhere, in a chapter entitled 'The Legacy of Social Darwinism', these 'highly restrictive and ancient aphorisms' appear more congenially in the guise of 'some very persistent and largely unchanging ideas about society':

> the importance of individual faculty in social affairs; evolution as the growth of rationality; the belief in the superiority of contemporary social systems; the idea that there exists a general law covering social and natural events; the notion of the 'natural' in man and its contribution to social structure; the importance of the natural and animal world as a reflection of the social; the use of analogies from nature. (p.176)

In the historical development of Social Darwinism, Jones points out, only the 'form' of these ideas has changed, and that largely in response to a shifting 'political constellation'. By 'form' she probably means the form in which the ideas were expressed: that is, their constituent language. By 'political constellation' she refers to large-scale configurations – 'Labourism', Communism, Nazism – that have intervened in the history of Social Darwinism since the late nineteenth century. The political constellation under which sociobiology has come to prominence does not concern her, but she offers evidence that the preoccupation of sociobiologists with 'social structure', 'altruism', and 'differential fertility' is only a demonstrable transformation of traditional concerns. Sociobiologists have sought conveniently to escape charges of Social Darwinism by defining it *à la* Hofstadter and others as merely 'a theory of competitive individualism and racial conflict'. 'They are, in fact', Jones declares (p.187), 'disguising from themselves their own heritage.'

Shades of evil precursors becoming saints. For very different reasons, I suspect, E.O. Wilson and Michael Ruse would now agree. Which returns us to the august halls of Harvard, where Wilson and Ruse have recently been ensconced beside the Marxian naturalists Richard Lewontin and Stephen Jay Gould, where 'biological determinism' is everywhere condemned and the 'end of ideology' has ended. Or so it would have seemed, with the 'political constellation' of the New Right in the ascendant. But from across the Harvard campus, from the Charles Warren Center for Studies in American History, has come within the last two years a studiously neutral and loftily intellectualist proposal for the investigation of Social Darwinism. It appears in a 70,000-word essay entitled '"Social Darwinism" Revisited', undoubtedly the most thorough and comprehensive analysis of the subject ever produced.

Donald C. Bellomy clearly understands that historians do not work in a political vacuum. And he writes historiography very well. His reaction, however, to the distorting effects of Hofstadter's and Bannister's political commitments – let alone the 'radical revisionism' of Greta Jones – is to attempt to place the study of Social Darwinism on a high intellectual plane, far above warring factions (pp.128–9). 'Ideas', he declares, 'are in fact what they [historians of the post-Darwinian 'universe'] must most often survey, not just

popular ideologies or reflex responses to social and economic convulsions.' (Note the persuasive use of 'reflex'.) Because Darwin provoked an 'intellectual crisis' among intellectuals, these ideas are 'probably' to be studied 'only in a later stage of scholarly reconstruction'. Although they may or may not be reciprocally related to social change, 'ties between social and intellectual history can never be assumed *a priori*'. Nor should the ideas be studied on any terms but their own – certainly 'not simply as antecedents of contemporary social and political arguments or fields of research, if our goal is to comprehend either the past or the present'. (Note the persuasive use of 'simply'.) Scientific ideas, in Bellomy's view, seem specially exempt, for they appear to be separable from social and political ideas in historic debates about Darwinism: 'issues of political commitment and class or national bias were clearly important, but so too were other ideas, including biological theories and philosophical expectations.'

This is obviously an unpromising perspective from which to 'comprehend' anything except a totally abstract and disembodied Social Darwinism. But Bellomy, despite himself, has advanced the subject in ways that should be of interest to the Left. For instance, he points out that the phrase 'Social Darwinism' did not come into use until after 1880. It appeared first in French in that year, then more influentially in Italian in 1882. Social Darwinism *per se*, however, did not become a matter of controversy until ten years later, and then almost entirely among European sociologists who advised and reported on contemporary political reforms in the context of a resurgent 'Neo-Darwinism' in evolutionary theory. Their audience consisted chiefly of professionals like themselves; their 'community of discourse' placed a premium on 'precise and elaborate...depictions of possible relations between biological and social change' (pp.64, 72). The community of discourse was decidedly international in scope, and thus in 1897, insignificantly, and crucially in 1903, 'Social Darwinism' came to America. There the subject remained a contemporary problem for social theorists until about 1920, when it went underground for two decades, only to surface as a problem for historical scholarship with the inception of Hofstadter's research.

Two Histories Revised

Two important points, it seems to me, emerge from this retrospect. First, the conceptual history of Social Darwinism lies suspended between two signposts, each marking the end of a worldwide slump. Social Darwinism was invented as a problem for social theory in the 1890s, when it became a matter of urgency to pacify and contain the demands of labour in newly expanding and restructuring economies. Fifty years later the subject became a problem for historical scholarship when in America it seemed urgent to contrast a *laissez-faire* past with the beneficial stabilizing and unifying effects of state intervention during the Great Depression and the Second World War. How 'Social Darwinism' mediated the impact of two great economic crises would be well worth investigating. Disciplinary histories of the social sciences and biology would have much to contribute; a history of 'socialist science', as Bellomy's evidence shows (pp.42ff.), would make for instructive if uneasy reading in this regard.

The other – and for my purposes more important – point to emerge from Bellomy's analysis is that 'Social Darwinism' is primarily a problem of our *own* making. I speak as a member of the professional middle class. The problem was formulated among sociologists and historians at different times and places. Initially it was a problem of theories and policies; latterly it became a problem of definitions and labels. (If a self-conscious Social Darwinism should arise under the influence of sociobiology, then the initial problem will return.) Historians, thanks to Hofstadter, projected the latter problem back into the decades immediately following the publication of the *Origin of Species*, as if 'Social Darwinism' had somehow been debated from the time Darwin's name became prominent. But the phrase was never used. 'Darwinismus' in Germany and 'Darwinism' in the English-speaking world quite sufficed to express all Darwin's intentions, all his allies' hopes, and all his critics' fears, without the gratuitous annexation of 'Sozial-' or 'Social' to the term. The routine distinction made today between 'Darwinism' and 'Social Darwinism' would have been lost on the author of the *Descent of Man*, and probably on most of his defenders until the 1890s (Fichman, 1984). Only then, with the 'revolt

against positivism' and the professionalization of sociology, was it demanded that 'biological' theories should be clothed in 'social' garments in order to move modestly through the new discipline. Only then, with this fragmentation of knowledge and its divorce from general culture, did 'Social Darwinism' become a problem – an artifact – of bourgeois perception.

For radicals the perception must become problematic. The problem of Social Darwinism is not to illustrate its history as an idea, or a congeries of 'ideas, values, and activities' in Bellomy's phrase, with a sordid succession of reactionary examples. That would be merely navel-gazing – scrutinizing one's own preconceptions. Nor is the problem simply to write a conceptual history of 'Social Darwinism', important as it will be to learn how the phrase, in its professional career, mediated social conflicts and resolutions. The problem for radicals is rather to reconstruct the common context of language and assumptions in which biological and social theories once intermingled, in which Darwinism *was* Social (Young, 1985a), and then to explain why this 'commons' was enclosed, how the language and assumptions were parcelled out among professional interests, and what impact this had on the masses of lay people who thereafter were barred from 'informed' debate on biological and social questions that affected their political destinies.

Here Bellomy, for all his clarity and documentation, is less helpful than Greta Jones. Faced with the welter of Darwinian allusions and catch-phrases emanating from the later nineteenth century, Bellomy resolutely erects a barrier between words and ideas (pp.29–30). Darwinian rhetoric was a 'badge of realism'; it represented a quest for 'order and unity' in the universe. But it 'played a very minor role in the creation of new ideologies'. 'One of the most seductive fallacies into which historians, especially intellectual historians, can fall', he warns, 'is the presumption that the language in which an idea is expressed is a reliable guide to the content of the idea.' For evidence against this presumption, Bellomy turns to the subject of his recent Ph.D thesis, William Graham Sumner. He proceeds to analyse 'the discrepancies between Sumner's meaning and Darwin's ideas' (p.36); yet he remains quite unconscious, it would seem, that Darwin's ideas were expressed in a language as well. But giving Bellomy the benefit of the doubt: since Darwin's language,

ex hypothesi, cannot be presumed to be a reliable guide to Darwin's ideas, we may suppose that Bellomy derived his knowledge of those ideas from somewhere else in order to compare 'Sumner's meaning' with them. And, of course, the evidence of the essay would suggest that this knowledge may have originated in a somewhat selective reading of the *Origin of Species* and the *Descent of Man* from the standpoint of Darwin's present-day defenders, some of them perhaps at Harvard.

Jones, on the other hand, is at least aware that language serves in the creation of ideologies in many important ways besides expressing ideas reliably. She recognizes that ideologies need not be deliberately thought out, but may be tacitly structured and strengthened through metaphor, ambiguity, and transposition of terms. It is ironic that Bellomy obliquely acknowledges her insight that 'Darwinism provided a versatile lingua franca capable of expressing an almost infinite variety of opinions on public issues' (p.25): this insight, though overstated by Bellomy, is the premise on which not only Jones, but Hofstadter himself long ago, began the investigation of Social Darwinism. Hofstadter wanted to know how ideas get a hold on people.He was interested in the mediation of scientific theories to the reading public and the transformation of biological concepts into social theories and ideology. He was interested in the use of words. Bellomy, however, having ridded Hofstadter's America of Social Darwinism, finds the subject 'of decisive importance at other levels of discourse, in other decades, in other contexts, in other countries'. That is to say, he finds Social Darwinism 'of decisive importance' only at the 'later stage of scholarly reconstruction' in which sociologists had invented the phrase. This strikes me as history from the top down and back-to-front. If 'Social Darwinism' was ideologically insignificant until professional people thought about it as such, what phrase shall we use to refer to the decades of public Darwinian social and political rhetoric that preceded the introduction of 'Social Darwinism' into the English-speaking world?

I maintain – no, I insist – that 'Social Darwinism' is redundant, for Darwinism *is* 'social'. I insist also that the phrase is a pejorative tag, and the Left must speak plausibly of what it condemns. Nevertheless we shall probably have to continue using the phrase, if only in

the interests of broad communication. I propose, therefore, that in doing so we should make every effort to salvage 'Social Darwinism' from the implausibility into which it has sunk through polemical usage, Left and Right, by remembering and qualifying ourselves appropriately on two crucial points: (1) 'Social Darwinism' has a *conceptual history* that reveals the phrase as an artifact of a professional discourse in which, increasingly, a scientific theory (Darwinism) was to be distinguished from its human (Social) implications; (2) Social Darwinism also has a *rhetorical history* that antedates the invention of the phrase and remains today the prime domain for comparing popular and elite perceptions of evolutionary biology, for studying the political appropriation of biological language, for tracing the dissemination of ideologies couched in Darwinian phrases – in short, for socializing Darwinism.

Social Darwinism as Social Darwinizing

Since bourgeois philosophy of science now frankly admits that 'Darwinism' is not a body of essential theory, a timeless truth (Hull, 1985), there can no longer be any excuse for regarding *Social* Darwinism as anything but a nominal construct. Generally speaking, Social Darwinism may be seen as a shifting congeries of rhetorical manoeuvres, with associated assumptions and arguments, that manipulate certain terminology, present in or bearing a family resemblance to that of Darwin's writings, for the purposes of describing or sanctioning social arrangements that owe their existence to historical rather than evolutionary forces. Social Darwinism is thus a way of speaking that conceals the reality of social action – Social Darwinizing, let us say.

Social Darwinizing has no *inherent* political tendency; it is neither Right nor Left, not liberal nor radical nor conservative. Its concern is not biology on the one hand, or society on the other, but the application of language about biological theories to society, and vice versa, within various political contexts. Social Darwinizing generates practical ideology by representing actual or possible social arrangements as somehow constrained by biological necessity. The medium of representation is, for the most part, ordinary language, and so it has ever been since Darwin expressed himself in the common parlance of his day.

Victorian people did not have our reticence about Social Darwinism, even if the phrase had not been coined. In popular speech and even in middle-brow culture the distinction between what was properly 'Darwinian' and what was (improperly) part of its social 'implications' or 'applications' had not been made, or if it had, it was not jealously patrolled by posses of philosophers and scientists. For instance, as early as 1863 letter-writers to *The Times* began referring unselfconsciously to 'Darwinian' conflicts between human beings.[6] In 1871 *The Times*, reviewing the *Descent of Man* during the siege of the Paris Commune, declared that 'a man incurs a grave responsibility when, with the authority of a well-earned reputation, he advances at such a time the disintegrating speculations of this book.' And on the afternoon of Darwin's funeral in April 1882 the *Pall Mall Gazette* bore eloquent testimony to the Darwinizing of society. Referring generally to Darwin's conception of evolution, the leader-writer reflected,

> Where it did not act directly it acted indirectly. Trickling and filtering down to the masses, it permeated even the daily press, the current political and social ideas, the beliefs and aspirations of the sects and the Churches. Those who themselves explicitly reject the Darwinian creed yet cannot possibly escape from many of its implications. It runs through almost all the best thought of our time; it tinges our unformed public notions; it reappears under a hundred disguises in works on law and history, in political speeches and religious discourses, in artistic theories and vague social speculations. Our very novels and poems are full of latent Darwinian gems. If we try to think ourselves away from it we must think ourselves entirely away from our age.

Even allowing for exaggeration, this is an impressive contemporary witness. If, however, people only *sometimes* found it difficult to think themselves away from Darwinian ideas, it was not because Darwin had stamped a new terminology on the collective mind but because he had used familiar words so well.

Darwin's candid impulses in the matter of English usage can be gauged from the unrevised texts of works he never published. In the 'big book' (1975) of which the *Origin of Species* was an abstract, for example, he referred to certain species as 'maiden knights who have not fought with each other under the great battle for life or death'. Old species supplanted by their distant offspring become the 'great

uncle' of the new species; each species is abundant in its 'metropolis', but species in the 'lowlands' may be 'invaded by highlanders'. 'Changed relations or associations between the inhabitants of a country, from opening up new places in its polity,...[are] the most important element of success', Darwin observes like an entrepreneur, while over all presides the 'economy of nature', which he calls 'the Police' (pp.180, 189, 202, 227, 245, 273).

In the *Origin of Species* many of Darwin's more colourful phrases were muted – the famous line remained intact about 'one general law, leading to the advancement of all organic beings, namely, multiply, vary, let the strongest live and the weakest die' – but he undoubtedly enhanced his rhetorical impact by condensing and streamlining the original, longer text. One of few uses for the *Concordance to Darwin's 'Origin of Species', First Edition* (Barrett *et al.*, 1981), which has been remaindered, is to check the frequency of words borrowed from social life. In the following list the incidence of particular terms and their cognate forms, where the usage is relevant, appears in brackets. The classification proceeds from organic units to their identities and qualities, and on to the nature and outcome of their interaction.

'individuals' (230), 'classes' (117 nouns), 'races' (62 nouns)

'parents' (206), 'slaves' (49), 'masters' (16), 'mother' (15), 'children' (14), 'savages' (14), 'natives' (13 nouns), 'foreigners' (9), 'father' (5), 'barbarians' (3), 'tribe' (1)

'civilized' (13), 'strength' (12), 'weakness' (4), 'uncivilized' (3)

'struggle' (86), 'competition' (71), 'battle' (13), 'attack' (13), 'defend' (7), 'war' (5), 'race' (4 verbs), 'weapons' (4), 'combat' (3), 'armour' (2), 'contest' (1)

'exterminate' (56), 'colonise' (16), 'beat' (14), 'kill' (14), 'victory' (14), 'conquer' (6), 'triumph' (3), 'overmaster' (3), 'slaughter' (2), 'win' (1)

Whatever else these terms may show, they denominate a world of competitive individualism, racial hierarchy, and imperial advantage. One could be forgiven, on this basis, for thinking the *Origin of Species* dealt with human evolution, so frequently – twice on average per page – does Darwin draw on the language of everyday social life to interpret the natural world. The point, however, is that the

book *is* about human evolution. And once Darwin's language was recirculated, now under the banner of biology, familiar terms acquired a fresh authority. People not only spoke differently – more often, say, about 'survival', 'fitness', and 'species' – but they thought differently about how they spoke and what they spoke about. Ideology had undergone a scientific translation; Social Darwinizing was the result.

Social Darwinizing Today

The process goes on with a vengeance in Reagan's and Thatcher's world (Rose and Rose, 1986). Social Darwinizing is not confined to sociobiologists. 'Aggression', 'altruism', 'promiscuity', 'incest', 'courtship', 'nepotism', and Wilson's 'leash' on which nature holds culture are merely the Darwinian 'retranslations' (Jones) of our time. In years to come perhaps these will have achieved the currency that original Darwinian phrases now enjoy. It is difficult if not impossible to discuss major public issues without using the familiar catchwords. Civil defence and famine relief are about 'survival' (27 times in the *Origin*), the 'nuclear winter' is about 'extinction' (167 times), educating children is about 'selection', animal liberation is about 'species', and corporate takeovers are part of the 'struggle' for commercial existence. More significantly – perhaps more ominously – the mass phenomena of jogging, slimming, and athletic competition are invariably associated with 'fitness' (37 times), fitness with survival, and survival with national pre-eminence.

Occasionally we are reminded that our language has a history. With Spencer's *Principles of Sociology*, as well as his *Principles of Ethics*, now reprinted in America, Robert Reich used his review in the *New Republic*, 'Ideologies of Survival' (1982), to deplore 'the return of social Darwinism'. From across the Atlantic, James Burke did a hatchet job on Social Darwinism in America ('cowboy ethic'), in Germany ('nineteenth-century pseudo-scientific garbage'), and in Russia (Marx a 'social evolutionist'), but *not* at home, in 'Fit to Survive' (7 May 1985), a programme in his BBC-1 series, 'The Day the Universe Changed'. Richard Dawkins of *The Selfish Gene* has, however, evoked Britain's imperial past in 'Nice Guys Finish First' (14 April 1986), a contribution to BBC-2's 'Horizon' strand, in an

ill-judged attempt to stop his title being misinterpreted: 'Maybe we need less of the rule of law than what Kipling called "the law of the jungle".' It was in Kipling's spirit, indeed, that Maggie's men went to die at Bluff Cove and Goose Green, whereupon Social Darwinizing received a new lease of life in the journal of the American Association for the Advancement of Science. Readers of *Science* were presented with an article by William J. Broad, 'Survival of the Fittest in the Falklands' (1982), that gave a purely historical account of 'Darwin's observations on the political and biological struggle for existence' during the visit of the 'Beagle' in 1833.

More recently historical Social Darwinizing has been disinterred in the effort to identify the body of Dr Joseph Mengele, the Roman Catholic SS officer and head of the human genetics laboratory at Dachau. A 'manuscript of scientific and theological studies relating to Darwin's theories on the origin of species' was discovered, according to *The Observer* (9 June 1985), that had been written by the 'Gerhard' later identified as the doctor himself. 'Mengele is the product of the kind of system we still have', declared a scientist interviewed in the Central Television documentary 'Mengele' (9 September 1985) – a view no doubt shared by the medical anthropologist Howard F. Stein. After the *Time* magazine cover story, 'The Fitness Craze: America Shapes Up' (2 November 1981), Stein published a provocative analysis, 'Neo-Darwinism and Survival through Fitness in Reagan's America' (1982). There he interpreted 'health', 'wellness', and related terms as metaphors of a 'neo-Darwinian nationalism' that differs little in principle from the romantic cult of social hygiene under the Third Reich. (Stein wrote two years before Reagan stumped the country declaiming, 'The United States was never meant to be a second-best nation. Like our Olympic athletes this nation should set its sights at the stars [!] and go for the gold.') No wonder Walter Mondale, in announcing his candidacy for the Democratic nomination in 1984, was moved to say, 'I believe in social decency, not Social Darwinism' (*New York Times*, 21 February 1983).

Usually, however, Social Darwinizing today takes place in a historical vacuum. Language floats free with a seeming life of its own, exacting acquiescence to the harsh demands of nature on history. Again 'fitness' and 'survival' are the recurring themes. Doctors de-

fend themselves on 'purely medical' grounds for terminating the kidney dialysis of a patient billed as 'unfitted' (*Guardian*, 8-9 January 1985). 'Ethopians hope to shift 1,500,000 people this year', according to the BBC's 'Nine O'Clock News' (11 March 1985), 'but it's very much survival of the fittest.' 'Regency Street is about the survival of the fittest – those who are not strong, just fall by the wayside', observes a staff member at the London social security benefits office near Victoria Station (*Guardian*, 27 April 1985). 'Some horses will be shot on the way', Lieutenant-General James Abrahamson, director of Reagan's Star Wars research, warns hopeful British defence contractors. 'Only the fittest will survive' (*Guardian*, 19 February 1986). Meanwhile survival skills are demonstrated by the Royal Marines' 'elite Mountain and Arctic Warfare Cadre' in a BBC-2 programme, 'Red in Tooth and Claw' (21 May 1985); 'Nuclear War and Civil Defence' are featured in the Channel 4 series 'Survive' (10 March 1986).

In America 'survivalism' is pursued for family, fun, and profit, according to an article in the *Sunday Times Magazine* (14 October 1984), 'The Four Salesmen of the Apocalypse'. (The apocalyptic motif is new in the history of Social Darwinism; it trades off the threat of nuclear 'extinction'. Ethel Tobach and others have seen Social Darwinism itself as apocalyptic in *The Four Horsemen: Racism, Sexism, Militarism, and Social Darwinism*, published in 1974.) Militant survivalists are wealthy 'conservative individualists' in their late twenties and thirties with families and small children. Fifty thousand of them subscribe to *Survive*, edited by a former American Nazi stormtrooper, a periodical that 'evolved' out of *Soldier of Fortune* and now serves as 'a kind of *Which?* magazine for the apocalypse'. They spend fabulous sums on fallout shelters, freeze-dried foods, and sophisticated weapons. They take physical fitness – 'preparedness' – very, very seriously. Fainter hearts with $35 or more to blow on a weekend settle for playing the 'National Survival Game', promoted as 'the fastest-growing sport in America'. This pits young lawyers and executives against each other in full camouflage outfits, battling with paint bullets in the backwoods outside American cities. It is 'a middle-class version of the post-nuclear scenario' imagined by militant survivalists, Rambophiles playing capture the flag.

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Survivalism comes to Britain: 'the sport of tomorrow' targets the corporate customer today (*Sunday Times* magazine, 8 June 1986). Whose future is this?

Teddy Roosevelt would have approved. His imperialist vision, emboldened through Presidential experience and war, has at last found its fulfilment in America after Nixon and Vietnam. 'A nation that seemingly dies may be born again', he declared at Oxford seventy-five years ago in his Romanes Lecture, *Biological Analogies in History* (1910, pp.38–9). 'Best of all is it...to see our blood live young and vital in men and women fit to take up the task as we lay it down; for so shall our seed inherit the earth.' Survivalism is only a variation on this theme. It is Darwin's 'better class of men' leading Roosevelt's 'strenuous life' to save their families from extinction. It is 'kin selection' exemplified, lifeboat ethics writ small. Some ten years have passed since Garrett Hardin, the Neo-Malthusian proponent of lifeboat ethics, and E.O. Wilson first vented their Social Darwinizing in America. Hardin uses lifeboat ethics to justify the defence and preservation of the highest attainments of human (read 'Western and Northern') culture, if necessary even at the expense of refusing aid to those proliferating inferior (read 'Eastern and Southern') cultures that threaten to swamp and overwhelm. Wilson, who with Ruse is currently placing Neo-Malthusian ethics on a sociobiological basis (Ruse and Wilson, 1986), uses kin selection to explain why people tend to behave more altruistically towards those more closely related to themselves – their 'kith and kin'. As Ruse (1986b, pp.241–2) puts it,

> We do feel we have special obligations to our children. To deny this is to make total hypocrites of us all. And after our children come our neighbours and countrymen...If you doubt me, ask yourself the following: is your own government grossly immoral when it puts more of your tax money into social programmes benefiting the poor and needy of your own country, rather than into foreign aid?

Both Wilson and Hardin have now been honoured with the National Medal of Science by Presidents of the United States. Although Reagan has shown himself 'soft' on creationism, let no one imagine that he, any more than Roosevelt, cannot recognize a rationale for Realpolitik when he sees one. America still has much to lose. Social Darwinizing still creates safe niches for selfishness on planet Earth. The meek will not inherit it unless they take it from the strong.

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'Evolution' promotes 'skin survival' in women: 'cosmetic scientists' achieve 'results that can be measured' in the fight against 'harmful free radicals' (*Sunday Times* magazine, 6 April 1986). Whose survival are they promoting?

For Left intellectuals the first step could be to unmask the selfish slogans of those whose bellies are full and whose bank balances are in the black. Of course Social Darwinizing on the Left must not be ignored. Arguments for a 'biosocial science with a Marxian focus', for instance, can be shown to belie themselves in so far as they draw on sociobiology to help establish the 'innate parameters' of human nature; for even if 'knowledge of human limitations is part of a positive emancipation', sociobiology is evidently the last place for radicals to look for guidance about what society 'ought not' to do (Heyer, 1982, pp.221–7). But perhaps the best way for Left intellectuals to undermine Social Darwinizing among themselves is to expose its real functioning and affinities for all to see, wherever it may be found. Its conservative tendency in today's world would then be evident; radicals might even unite in the wider struggle against the disinheritors of humankind.

To put it compactly: the application of biological language to social relations tends to give Nature (unmediated) the edge over history. In common parlance such applications inexorably structure social expectations and invest the interpreters of biological language with Nature's authority when they pronounce on historical contingencies. Social Darwinizing thus becomes ideology in action. But to complete its exposure, Left intellectuals also need to show that the distortion and obfuscation of social reality does not originate simply in biology, in the coded incantations of a priestly caste of evolutionists, but also in the sometimes strategic, sometimes stylistic, appropriation of ordinary language to make biological comment. Genes become 'selfish', bugs may be 'promiscuous', birds engage in 'courtship', and the language of human relations acquires a specious inevitability by virtue of its incorporation into the lexicon of science.

This language, then, is popularized. Meanings traffic on a two-way street. People come to think differently about the familiar words they use and about the subjects to which these refer. This 'conjuring trick', as Engels called it, is not infrequently accomplished by biologists, perhaps especially by those who seek general recognition for the relevance of their work in a controversial environment. Darwin was, in this sense, the outstanding conjuror of all time. His linguistic legacy is most evident today among the

commentators, pundits, and activists who concern themselves with the 'fate of the earth' and its inhabitants.

It would be wrong to fault the participants in 'Band Aid' and 'Live Aid' for Social Darwinizing in their efforts on behalf of the 'survival' of African civilizations; it would be churlish, though less clear-cut, to ask whose 'fitness' the participants in 'Sport Aid' ultimately cared about. The point, however, is a deeper one. If efforts to save the human race are to avoid merely salving the collective conscience of the powerful or, worse, devolving in the hands of their governments into the discriminate charity of the age of Malthus and Darwin, or, worse still, into 'lifeboat ethics', then a critical check is needed on the terms in which these efforts are explained and justified before the public. The precipitous descent from 'Protest and Survive' to 'Protect and Survive' to crass 'Survivalism' stands as a warning. Selfish apologists for selfish systems would lure us over the brink through Social Darwinizing. By socializing Darwinism we may resist.

Acknowledgements

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Notes

1. Most of these references came to me by courtesy of Ralph Colp, Jr, whose writings on Darwin and Marx deserve to be better known (see References).
2. Darwin devoted by far the larger portion of the *Descent of Man* to expounding his theory of sexual selection. Greene does not deal with this part of the book, but Eveleen Richards has done so brilliantly in an essay (1983) that reveals the personal and cultural sources of Darwin's sexism, although she stops just short of calling it Social Darwinism.
3. Students are set the exercise in my essay (1980) for the Open University course A 309, 'Conflict and Stability in the Development of Modern Europe, 1789–1970', from which portions of the following paragraphs have been adapted.
4. Before the advent of sociobiology Christopher Nichols (1974) regretted that a

true 'Social Darwinism' had not emerged in the nineteenth century as a precursor of Lionel Tiger and Robin Fox's evolutionary ethology.

5. To illustrate the pace of events: in lectures delivered in August 1975 W.J.M. Mackenzie (1978) declared confidently that biological 'orthodoxy' had 'totally discarded Social Darwinism, in the sense that virtually no one sees genetic selection as a major continuing factor in human history now' (p.38). Wilson's *Sociobiology* was just appearing and Dawkins's *Selfish Gene* was in the press.

6. I am grateful to Frank James (1984) for pointing this out to me.

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DIALECTICAL BIOLOGY AS POLITICAL PRACTICE
Looking for More Than Contradictions

Peter Taylor

Essay review of Richard Levins and Richard Lewontin, *The Dialectical Biologist*, Harvard University Press, 1985, 303 pages, hb $20, £18.50

Dialectics has earned a dubious reputation among political radicals, scientists and Marxists. To radicals, strident and doctrinaire invocations of dialectics are one form of 'correct-line' politics. Political debate has been inhibited by those who, claiming the privilege of loyalty to the proletariat, detect error wherever they hear a questioning voice. To scientists, the scientific failure and political repression stemming from Lysenkoism proves the bankruptcy of attempting to inform scientific work with political critique; scientists are thereby relieved of feeling any responsibility to be self-reflexive about the social basis of their own work. To Western Marxists, there is little content in arguments that nature is dialectical. Even for the social realm where capitalism is held to create contradictions, Marxists have difficulty deriving from Marx a dialectical method to expose those contradictions.

Given such a problematic background, we might advisedly steer away from dialectics as a guide to the intellectual and practical issues facing would-be radical scientists. Yet when Richard Levins and Richard Lewontin – two Marxist professors at Harvard – advocate a dialectical biology, this invites us to take another look. Are they rehabilitating the old programme of 'diamat' (dialectical materialism) or breaking new ground in science? Do they argue that nature really is dialectical or just that our interpretations of it should

be? How does their work help us understand complexity in evolutionary biology and ecology? And, most importantly, does their dialectics help them to reconcile being politically committed, working as scientists and being privileged intellectuals?

That Levins and Lewontin's (L&L's) work should even bear on these issues is remarkable in the academic world of the United States. The scope of the topics they address is impressive; their writing flows wonderfully; their arguments are accessible and engaging. Nevertheless, I will argue that they have stopped short of, or deflected, difficult questions of practice.[1] The political dimension of their work relies heavily on the possibility of mobilizing science for liberation in the Third World, while their mainstream scientific research is limited by their remaining both pragmatic about, and well integrated into, the social structure of science at the 'centre', in the Scientifically Dominant World. Radical scientists ought to say a lot more than they do about stretching what is possible at the centre, where most of us live the 'social relations of science' (Young, 1977).

Before looking at L&L's work in detail, I should mention that they do not refer to contemporary or past debates about Marxism, dialectics and science. They show little sign of responding to or being challenged by issues such as whether dialectics is epistemological or ontological or relational, or whether contradictions are universal or specific to capitalism, or whether it is possible to define rigorously the content of the laws of dialectics (transformation of quantity into quality and vice versa; interpenetration of opposites; and the negation of the negation (Engels, 1927)).[2] L&L do not try to cut their way into the thicket of the intellectual and political debates, predominantly European, about these issues. Their audience is elsewhere – broadly speaking, in biology. L&L's dialectics is an accessible, seat-of-the-pants dialectics. With this they hope to deal with complexity, to build an anti-reductionist biology, and to connect their politics with their science. It is on these terms that I will review their work, even where I think their concerns could have been expressed without invoking dialectics. Nevertheless, as I hope to make clear, L&L's project has various ambiguities and tensions which they could have worked through by engaging in wider debates about dialectics. In particular, when they appeal to correct-

ness in accounting for nature, they miss many opportunities to contribute to a more profound politics of scientific knowledge, which, like all other knowledge, is inescapably mediated by historically specific human labour.

There are, in turn, various unresolved ambiguities and tensions in my essay. For five years I was a student of L&L, having chosen to work with them because of their combination of biological and political thought. It is out of respect for their work – both as represented in *The Dialectical Biologist* and elsewhere – that I have paid so much attention to it. Where this review is critical of their work, the implication is not that I would have avoided their shortcomings. Instead – and this involves a partial forgetting of the context in which their work developed – I have used the publication of their essays as an opportunity to explore, from a vantage point in the mid-1980's, the task of trying to inform scientific work with social critique. The limitations of this review are, in part, the limitations of defining one's identity through criticism of others instead of through examination of how one's own practice could be any different.

Critique of Scientific Categories

In the first two parts of their book – 'On evolution' and 'On analysis' – L&L address many important theoretical issues in biology without burying the issues in technical terminology.[3] One of L&L's most important arguments is that evolutionary theories are pale caricatures of the relationship of organisms to their environments. This is, in part, because those theories stand in broad ideological correspondence to contemporary socioeconomic relations:

> ...modern adaptationists regard the existence of optimal structures, perfect adaptation, as evidence of evolution by natural selection. There is no progress because there is nothing to improve. Natural selection simply keeps the species from falling too far behind the constant but slow changes in the environment. There is a striking similarity between this view of evolution and the claim that the modern market society is the most rational organization possible, that although individuals may rise and fall in the social hierarchy on their individual merits, there is a dynamic equilibrium of social classes, and that technological and social

> change occur only insofar as they are needed to keep up with a decaying environment. (p.84)

A dialectical perspective enters L&L's critique at times casually, at other times forcefully; it is central in their discussion of the 'interpenetration' of the categories organism and environment. They argue that organisms should not be viewed as simply responding, through behaviour and development, to some given environment. Nor should organisms be viewed as passively filtered by the selective forces of the environment (Neo-Darwinism). In a dialectical perspective, organisms select their environment, modify it, modulate its variation, determine what aspects of the environment are relevant, transform internally the physical form of environmental signals, respond to the environment; in short, within limits the organism constructs its own environment.

A dialectical perspective enters also in their discussion of the interpenetration of the categories biological and social in human life. In the theories of biological determinists the biology of individual humans, established in the evolutionary past, is relatively unrefracted by social life. In contrast, L&L make the simple but pertinent observation that, although our physiological needs are similar to those of other mammals, we can – through labour and exchange – obtain clothing and shelter to mitigate the effects of climate. This at the same time creates a vulnerability to economic and social conditions. Similarly,

> food today can be turned into food tomorrow by storing it in the form of social obligations that do not deteriorate with moisture. But once the food can be represented symbolically as obligations or money ...accumulation no longer has natural limits imposed by the body's weight or the physical problems of storage [and], while non-commercial redistribution is a protection against the uncertainty of the environment, trade creates new sources of uncertainty. (pp.261-2)

> The diversity of meanings of actions that seem superficially to be biological acts...also shows that a [Cartesian] causal analysis of such acts must be incorrect. People do not eat because their genes tell them they must eat in order to survive, since the same person in single day will engage in different 'eatings'. Nor do they eat because there is a general law of social organization that dictates eating as the appropriate response to the desire for social intercourse. The well-off authors of this

> book do not often use meals as social occasions...while people living on welfare can never do so even when they want to [at least, not in the same way, P.T.]...The individual and the social interpenetrate each other; the individual life history is the particular pathway that the realization of forces takes, but the individual can develop only in the context of a social milieu. (pp. 263–4)

Expressed in such moderate terms L&L's dialectical perspective might seem obvious, at least unthreatening, even to the 'Cartesian' biologists (Neo-Darwinists, biological determinists, etc.) whom L&L are challenging. Their nontechnical prose ensures that their critique is accessible, but it also makes their critique unspecific. Everyone, they imply, should expand their thinking, but no particular research strategy is decisively undermined. One exception, perhaps, is the use of the Analysis of Variance: '...we suggest stopping the endless search for better methods of estimating useless quantities'(p.122). L&L's critique may be 'controversial' and a 'stimulus for discussion' – as Slatkin says on the cover blurb – but, in their own words, they have given us a 'sampler of a mode of thought,' not 'a single coherent project'(p.viii).

L&L offer few *products* of this mode of thought. One example is the theory about the evolution of organisms that construct their own environment, which they say could start from the picture of coupled differential equations (p.105):

Change in organisms (natural selection) $\frac{dO}{dt} = f(O,E)$

Change in constructed environment $\frac{dE}{dt} = g(O,E)$

where O and E represent organism and environment.

Unfortunately, they take this picture no further, leaving unexamined the issue of whether the complexities of organism-environmental configurations can be reduced to quantitative terms. For ecology, they present Levins's loop analysis and time-averaging – mathematical techniques that provide a qualitative estimate of how change in one part of a system ramifies other parts (Ch.6; also Puccia and Levins). These techniques do not lead to powerful treatments of actual complexity because they are, in practice, informative only for very simple systems. As didactic devices, how-

ever, they serve to reinforce L&S's dialectical picture. L&L show that interventions can, through interactions among parts of a system, result in counterintuitive outcomes. Similarly, L&L show that statistical methods are confounded by causal interactions; from this follows the lesson that statistical claims about causality indicate an idealistic view of causality.

There is one aspect of L&L's dialectics that, if taken seriously, is more difficult to assimilate than the examples I have mentioned so far. L&L dedicate their book to Frederick Engels, implying what their critique of scientific categories appears to corroborate: they believe in a dialectics of nature. That is to say, contrary to the prevailing wisdom of Western Marxists but following Engels, dialectics is 'the science of the general laws of motion and development of nature, human society, and thought' (Engels, 1878; 1907, p.173). Engels emphasized dialectical leaps, L&L the interpenetration of opposites. Nevertheless they are united in the claim that nature really *is* dialectical.

Notwithstanding their explicit Engelian sympathies, L&L's critique of categories does admit a less Engelian interpretation. They may merely be advocating a scientific strategy: if scientists adopt a dialectical perspective then they will be better able to see ways to capture the complexity of, for example, organism-environment configurations in ecology and through evolution, because they will not become stuck in the bogs of reductionism and reification. (This strategy calls for some scrutiny; I will come back to that later.)

In either case, whether we interpret coupled differential equations, loop analysis and time-averaging as elements of a dialectics of nature or as part of a strategy for investigating the complexity of nature, these products of L&L's dialectics hardly compel us to change radically either our world-view or our scientific practice.

Critique of Scientific Practice

The third part of L&L's book – 'Science as a social product and the social product of science' – stands in contrast to the moderate claims I have discussed so far. L&L's research has expanded beyond ecology and evolutionary biology to include the political economy of

agriculture and public health. In these areas they do not merely address the categories of science but make an explicit and forceful critique of scientific practice: 'Modern science is capitalist science.' (p.197). Science has the dual, contradictory character of allowing us to achieve an increased understanding of nature and at the same time being a social (class) product. Against the colloquial definition of the scientific – public, objective, disinterested, cooperative, international – L&L juxtapose an alternative definition – 'elitist, sexist, secretive, linked to the Pentagon, producing commodities for private enterprise' (Levins, 1982).

L&L's work on the political economy of agricultural research (Ch.9) supports the thesis of a strong link between agricultural research and capitalism's pursuit of profit and social control. They are not, however, suggesting that the link is simple. Capital has not, by and large, entered agriculture as corporate farming. L&L argue that the penetration of capital comes through control outside the farm gate of inputs and produce. In the USA the proportion of inputs produced on the farm has decreased from an average of 70 per cent to 30 per cent from 1910 to 1975 (p.210). 'The chief consequence of technological innovation to increase on-farm productivity has been to make on-farm productivity less and less important in determining agricultural value' (p.213). The total value added on the farm is now only 10 per cent of the total; the remainder is added in production of inputs (fertilizer, seed, machinery, etc.) and processing, transportation and marketing of farm produce. With these changes, farming has become a small and relatively powerless part of agricultural production.

Agricultural research workers in state agricultural experiment stations come generally from rural backgrounds and are genuinely motivated to make farming more profitable, less risky and an easier way of life. These researchers hope that food will thereby become cheaper and feed the hungry. Although this represents a naive attitude towards capital, it is not a servile one, because the researchers are actually responding to *farmers'* demands. Nevertheless, as L&L point out, because those 'demands are determined by the system of production and marketing...the farmer becomes the agent by which the providers of inputs and the purchasers of outputs use the socialized establishment of research' (p.214). '... science and other

aspects of social life interpenetrate...scientists, whether they realize it or not, always choose sides' (p.5).

Many of L&L's prescriptions for changes in agricultural research have a moderate dialectical tone similar to that of their critique of biological categories. Agricultural planning must have an overall ecological perspective, integrating knowledge about the cultivated land and its surrounding environment. Planning must include multiple goals – natural and social demands are inseparable. Nature is complex; attempts to homogenize it do not work: '...any intervention in the rich network of interacting variables is likely to have many indirect and unexplained consequences, some of which negate the original purpose of the intervention' (p.219), e.g. the secondary effects of pesticides. In contrast to high-technology, capital-intensive agriculture, they envisage a 'gentle, thought-intensive technology' (p.220) incorporating local knowledge. It would aim to establish 'polycultures, mixtures of plants that jointly maximize use of solar energy, have different nutrient requirements, suppress weeds, discourage pests, attract predators, and maintain favorable soil and above-ground microclimates,' (p.221).

When L&L consider where this vision of an ecological agriculture is to be realized, they prescribe a radical change in scientific orientation. In ways such as those mentioned above, agricultural research has been and will be constrained under capitalism. In the Third World, however, where both high-input agriculture and, we would hope, the economic relations are unsustainable, a revolutionary science might develop. This revolutionary science could recognize the dual, contradictory character of the science it derives through both its past colonial relations and the present dominance of the Western world.

> It is our thesis that many of the critical theoretical issues – the class versus universal nature of science, its relation to other kinds of knowledge, the role of the dialectic in natural science and of class struggle within science – which are treated as philosophical problems in Europe, will be fought out in the Third World countries as part of the political struggle for complete, real independence and as part of the struggle to build science in socialist countries. (p.227)

Scientist as Partisan

In writing about agriculture and public health, L&L have presented a clear model of scientific practice. Scientists choose sides, even if they do so unconsciously. Because science under capitalism has its dual contradictory character, radical scientists can best reconcile their interest in science and in human liberation by shifting their work to where the sides are clear, by becoming partisans in revolutionary struggle.

L&L's stance should not be dismissed as an academic pronouncement. Both men have histories of scientific partisanship. They openly acknowledge their Marxism; as a consequence, many in the US mainstream dismiss their scientific critiques for being 'political'. To radical scientists in the USA (and wider afield), on the other hand, L&L's combination of open commitment and scientific critique makes them an inspiration and a model, in particular for members of Science for the People and the New World Agricultural Group (NWAG) (see Boucher & Nabi, 1985). In these ways, comparisons with the 'visible college' of British scientific socialists – Bernal, Haldane, Needham, Hogben and Levy (see Werskey) – would be justified. Of course, in terms of wide social visibility and influence, there is almost no comparison: the past was another country.

Levin's scientific partisanship has a long history. He grew up in New York in a culture where, as he describes it, he took it for granted that he would be 'both politically active and a scientist' (1983). He has been involved in scientific solidarity work in Cuba, Vietnam, and most notably, through many students (and students of students) forming NWAG, in Nicaragua. His political/scientific talks (e.g. chs 8,10-12, and part of 9) are always engaging; he breaks taboos by discounting the 'colloquial' definition of scientific and by weaving webs of connections and suggestions about social forces influencing the content of science.

Lewontin's political activism is (comparatively) more recent but better known, through his critiques of biological determinism – first, of Arthur Jensen's work on the purported genetic basis of racial differences in IQ scores,[4] and later, of E.O. Wilson's sociobiological accounts of human social behaviour (e.g. Lewontin,

1980). More recently he has challenged research directed towards screening workers for susceptibility to hazardous chemicals instead of reducing workers' exposure to those chemicals.

Levins and Lewontin have also attempted to establish democratic work collectives within an otherwise hierarchical university organization. Although these collectives were not particularly successful (Lewontin, 1979), L&L continue to provide some invaluable institutional legitimacy as a base for politically motivated workers, especially when their work is about the Third World.

From Dialectical Perspective to Practice

> The value of the dialectic is as a conscious challenge to the major sources of error of the present, and our own description of dialectical principles is specifically designed to help solve the problems we work with in both our science and our political lives.(p.268)

L&L's scientific and political work is admirable, yet their model of partisan science leads us into some difficulties. If both the contradictions of capitalist science and the role of the dialectic in natural science will be worked out in revolutionary societies,[5] can there be a practice of dialectical, or even politically informed, biology with the Scientifically Dominant World? If contradictions are inevitable under capitalism, can we none the less use dialectics consciously to negotiate the contradictions and to direct our scientific work? With these difficulties in mind, let us look at L&L's dialectical strategy for doing science.

First, we should note that L&L's scientific partisanship has definitely influenced some of their own scientific work, within the hardly revolutionary United States mainstream. Lewontin's critique of the statistical procedure, the Analysis of Variance (ch.4), was provoked by Jensen and others using the A.O.V. to claim that they could separate the relative effects of genes (80 per cent) and environment (20 per cent) in the inheritance of intelligence. Lewontin saw Cartesian science underlying the attempt to separate causes. His alternative is reaction norms (p.114ff.) – descriptions of the variation in some character across a range of environments arranged in order of, for example, temperature or humidity. Levins chose to study genetics at college in the late 1940's

because as a young communist he was excited by the possibilities of a dialectical approach to science. His interest in Lysenko's arguments about the role of development and of physiology in an organism's response to environmental stress led eventually to the studies of plasticity and evolution in heterogeneous environments for which he is best know to biologists (1968).

In both these instances, and in L&L's critique of scientific categories in general, their view of nature is one I find to be richer, more complex and more appealing than the work they criticize. Nevertheless we must ask: how have they applied dialectical principles to arrive at their picture of nature? Dialectics has a reputation for being unhelpful to scientists; unless L&L have a systematic approach to dialectical science they allow that reputation to stand (see also Young, 1978).

It is informative to consider the way they reject the dogmatic application of diamat during the Lysenkoist period in the Soviet Union (ch.7): 'To attempt to...distinguish competing theories of physical events or to discredit a physical theory by contradiction is a hopeless task. For every point of genetics supposedly contradicted by dialectical materialism, we can show that in fact there is complete support' (p.192). L&L believe that Lysenkoism's programmatic use of dialectics in the realm of nature was misleading and flawed. Nevertheless they do not dismiss dialectics of nature; in fact they claim that they can support with dialectics every point of genetics that Lysenkoists dismissed. For example, 'developmental genetics ...asserts that a given phenotype [character] will result only if the genes of the organism are operating in an appropriate environment, but only some genotypes can have that result, no matter what the environment' (p.193).

L&L seem to be confirming what sceptics have often claimed, that is, that almost anything can be an example of dialectics (see also Engels). Furthermore, since the facts of genetics were not derived by any conscious dialectical method, you need not have a dialectical perspective to produce dialectical science (see also Trotsky). Nevertheless, we could extract a method for being *conscious* dialecticians out of L&L's comments: adopt dialectics as an overview and a set of warnings – the scientific strategy I mentioned earlier. Then, to lend weight to that strategy, show that dialectics is consistent with the known facts of genetics, or whatever:

> [Dialectics] tells us, 'Remember that history may leave an important trace. Remember that being and becoming are dual aspects of nature. Remember that conditions change and that the conditions necessary to the initiation of some process may be destroyed by the process itself. Remember to pay attention to real objects in space and time and not lose them utterly in idealized abstractions. Remember that the qualitative effects of context and interaction may be lost when phenomena are isolated.'(pp.191-2)

I want to explore some reasons for dissatisfaction with this formulation of dialectics. L&L do not describe any well-developed approach of analysis and resynthesis to expose the dialectical relations that determine the totality under study.[6] They simply tell us to remember many things; the model implicit in their method is that, provided we keep the dialectical checklist in our heads, we will be able to put dialectics into our natural science. This model also has a flipside: when we are critical of certain work, we can attribute that work's weaknesses to its author's failure to remember those dialectical guidelines.

Employing dialectics as after-the-fact critique, while calling it a method, places the emphasis on what a field of science ought to *look* like. This diverts attention away from whether and how those criticized – or the critic for that matter – could actually make the field come to look that way, given the contemporary organization, support and practice of science. Blurring of critique and method facilitated the correct-line attitude of diamat and Lysenkoism that is preserved today in particular sectarian Left debates (e.g. Felberbaum). L&L's dialectics does not have that danger; there is no central power endorsing their checklist. In their case the guidelines come to look more like a set of moral imperatives. Ecology, for example, 'must cope with interdependence and relative autonomy, with similarity and difference, with the general and the particular, with chance and necessity, with equilibrium and change, with continuity and discontinuity, with contradictory processes' (p.160).

Moral imperatives are a weak basis for method, reminding us of the idealism of the counter-culture: if we only get our heads straight (unstraight?), we can change the world. Sometimes L&L slip from idealism into dogmatism (see e.g. Lewontin vs Witelson, 1985); yet, at other times, we may discern a certain ambivalence about their

own project, a reluctance for us to hold their own scientific work up to the standards they set. They begin by telling us they have consciously worked in the dialectical mode and that their scientific books are a confirmation of the power of dialectical analysis (p.viii); yet they conclude: 'Dialectical philosophers have thus far only explained science. The problem, however, is to change it' (p.288).

But, of course, L&L are not alone in making prescriptions that are difficult to fulfill (see e.g. Young, 1977); there is a more important reason for drawing attention to the correct-line connotations of L&L's method. Prescriptions, given without much analysis of practice for realizing those prescriptions, make it all too easy for scientists to reject the dialectical project, to dismiss its proponents as moralistic, dogmatic, unhelpful and so on. Moreover, scientists extend this rejection to any direction of scientific work according to firm and explicit philosophical or political principles (Maynard-Smith, 1986). Once they have rejected any explicit commitment, scientists are 'free' to follow 'a path of least resistance' (p.2), thereby responding implicitly to dominant social relations. Facilitated by those relations, scientists incorporate them unwittingly into their science.

Instead of withdrawing from political commitment in their science, radical scientists should push further their commitment to transforming the Scientifically Dominant World. To do this without either reviving *a priori* philosophical dogmatism or fixating on standards of appearance, we need to take seriously the materialist claim that theory is shaped by the practices of science. Just as literary criticism analyses the production of literature, it should be commonplace for us to scrutinize the frameworks – social and economic, narrative and linguistic, psychological and political – within which scientists construct their work; these frameworks shape their representations and interventions in the material world. For scientists to have much chance of informing science with social critique, there will need to be a vigorous culture of criticism surrounding that science, with criticism reaching even to the points of origination of scientific activity and production.

Materialist 'science criticism' is just what I would want of dialectics. Not a debate about whether nature *is* dialectical or mechanical; nor about whether a dialectical strategy of science is better than

an analytical approach. Undoubtedly analysis works: experimentation on controlled, isolated systems is a powerful way of knowing the world. But the power derives from a social construction of reality – through engineering of systems it becomes meaningful *not* to reinsert systems, which otherwise would be open and transient, into their varied and variable contexts. The task of dialectics as criticism is to subvert this engineering of reality. On the one hand, it can pay attention to the modes of facilitation of the practice of science that allow scientists to stop at an analytical construction of reality. And, on the other, it can inform action that might reinsert science into its locally and historically specific discursive orders. Criticism and action together are needed both to make resynthesis possible and to see engineering as a problematic means of shaping reality.

I want, in the rest of this essay, to make a small contribution to this science criticism. I will explore how L&L's science is constrained by the material conditions of its production in the Scientifically Dominant World and consider some of the difficult questions of practice created by those conditions.

Evolutionary Biology in Practice

L&L often write and talk about the need to develop an evolutionary biology centred on the interpenetration of organism and environment. Let us consider how their experimental and theoretical practice bears on that project.

The work of Lewontin's population genetics laboratory has been to characterize the extent of genetic variation in nature at finer and finer levels of resolution – through detection of enzyme variation by gel electrophoresis to the current work on DNA sequence variation. Lewontin describes this as '*reduction* as a tactic of work', in contrast to '*reductionism* as a world-view' (1982). This implies that when in his lab he looks for 'God in the details', he is resolving issues incomprehensible to biologists without a dialectical world-view. This is a striking implication, but it is one L&L have not defended explicitly in these essays. If there is dialectics behind the choice of problems that Lewontin tackles with reduction, for the most part that dialectics is not made visible.

In fact, it is hard to see where Lewontin's population genetics is made any different through dialectics. Knowledge of DNA variation is helping to resolve questions evolutionary genetics had posed itself (Lewontin, 1985). Without going into detail, most of these questions derive from a time – not so long past – when evolution was both defined as, and held to be explicable in terms of, changes in gene frequencies. The issues of evolutionary genetics are a long way from questions about organisms constructing their own environments or other issues deriving from a dialectical world-view. Lewontin has remarked that the future for evolutionary studies lies in elucidating the coevolution of environment and gene-pool (1982); but if we are to judge by his practice, this elucidation is being postponed until the future.

In general, L&L's work on evolutionary models complements the experimental work just described. In contrast to their dialectical critique, L&L's models follow the convention of inventing a property, 'fitness', so that they can assign different values of fitness (or selection coefficients) in any given environment to particular variants of some unit of selection, usually the pairs of genes. These models create a demand that is met by various experimental manipulations and by statistical methods. For example, by skilful crossmating the population geneticist can construct populations that differ for very few genes. In (artificial) selection experiments, population geneticists mimic a nature that is 'single-minded' about the attributes required for the unit to be awarded differential representation in the future. Population geneticists then adopt the Central Conflation of statistics; that is, they fit data to a model retrospectively but act as if the data were really *generated* in the same fashion as the model assumes. This conflation makes it possible sometimes to assign values to the fitnesses (and other parameters such as mutation rates).

Given these models and the complementary technical practices, characters become merely the 'phenotypes' corresponding to the underlying inherited genotypes. In the search for genetic variation that corresponds to a model of differential selection of genotypes, the exact characterization of variation at the genetic level (Lewontin's experimental programme) seems most important. In

turn, the development of characters remains a minor consideration in evolutionary biology, or, at least, a consideration postponed until developmental biologists know a lot more.

The models, experiments and statistical analyses reinforce an image of nature that is mechanically materialist: natural selection is a force; the frequencies of the different versions move in response to that force. The environment is the source of power; the organisms are passive, in fact doubly passive. In the statistical picture of evolution, 'no moth alters its coloration; the coloration of the population evolves' (Sober, 1984, p.204). In this picture it is irrelevant that organisms construct responses which in new circumstances can be novel responses. Instead it is axiomatic that the heritable part of any new variation among the organisms is undirected with respect to environmental stresses. Darwin, not Lamarck; Descartes, not Engels.

Therefore it is not simply a Cartesian[7] world-view, but a set of experimental and theoretical practices, that makes it possible to know the natural world as composed of individual, mechanical objects. An alternative practice is needed to support an alternative evolutionary biology. Biologists could turn their attention away from the special cases where nature is single-minded, the rare cases where selection is an apt metaphor, e.g. grasses surviving on mine tailings despite the heavy metal levels (Antonovics *et al.*, 1971). (Levin's models of evolution in changing environments (1968) were an attempt to do just that without abandoning the mechanistic quantity, fitness.) Most evolutionary transformations are, like historical change, some intersection of structure and contingency. The structures of the internal (developmental) and external (environmental) relations of organisms both constrain their responses to that contingency and provide the flexibility for those responses. Perhaps evolution as a study of historical change requires some non-Whiggish historical method, recognizing regularities in developmental-environmental configurations, in place of statistical analyses connected with simple deterministic models.[8]

In order to develop an alternative practice, biologists might also need to extend L&L's critique of categories, to undermine certain categories around which theories and experiments are formed. Categories that seem 'real and concrete' may require analysis and

resynthesis into new 'totalit[ies] of many determinations and relations' (Marx, 1858). I am thinking in particular of the category of the individual. In the context of psychology and biology Chris Sinha has argued that the 'subject is "ex-centric". The biological individual is not the source of subjectivity but one support for it, a subjectivity whose actual structures of production and reproduction are to be sought in multiform material...practices.' Cathy Urwin has put such a view into practice in her observations and interpretations of power relations and the emergence of language in young children. In place of the unitary subject unfolding its innate competence and acquiring universal traits, she shows infants developing by positioning themselves within some historically specific discourse. She interprets the individual-in-discursive-order rather than maintaining some dualism between competence and performance. Evolutionary biologists might read: some dualism between genotype and phenotype.

Of course, these are only suggestive, and themselves problematic,[9] extensions by analogy from the study of humans. The individual will not be displaced easily from the centre of attention while current social and scientific categories reinforce each other. There is historical consistency in the way evolutionary biology after World War 2 has both been obsessed with the fortunes of the individual and the returns it gains from its investments (Haraway), yet at the same time has collapsed the external relations around those individuals into a unitary force of selection. The parallel in the social world is that the human individual's ties of personal dependence, e.g. within the family and community, have loosened, but the individual has become all the more subject to a technocratic rationality. Current evolutionary biology – the practice, the theory and the ideology – seems, therefore, very apt for the 'culture of narcissism' (Lasch), so much so that the transformation of evolutionary biology becomes linked into the project of transformation of our dominant psychoculture.

Simple Models and Heterogeneous Complexity

It is hard to detect a dialectical method behind the practice of L&L's evolutionary biology. In Levins's work in theoretical ecology, how-

ever, he does make some explicit comments about dialectics and method. These suggest a more noteworthy approach to practising dialectical science in the Scientifically Dominant World.

At first sight, Levins's research strategy, both in its method and in its product, looks consistent with his dialectics. He shows how complexity confounds predictions derived from simple causal pathways, and talks of the need to find where complexity resolves into higher-level properties that transcend the taxonomic particularities of ecological communities (p.140). Levins's strategy of model-building – models are his scientific apparatus – reflects his belief that contradictions are central to the activity of theory production. Models are not proposed to be true representations of reality; instead they are necessarily 'false, incomplete [and] inadequate' (1966) – useful for a time, particularly when their discrepancies from observations provoke interesting questions, but eventually such 'contradictions' (1982) ensure that any model is 'outgrown when the live issues are not any longer those for which it was designed' (1966).

Ecological complexity is unlike the statistical complexity of physical and chemical sciences. Two individuals of the same species will differ in age, sex, genetics, experiences and so on. Populations of different species will be distributed on different spatial scales and change on different time-scales. To deal with the heterogeneously complex relationships of ecology, Levins does not advocate building complex, detailed models which might capture the particular detail but which would have limited generality. Far from it: Speaking through Isadore Nabi, in chapter 5 L&L ridicule systems analysis and multivariate statistical analysis. For L&L, attempts to include in advance all conceivable variables result in unmanageable and unproductive models. This outcome can be easily explained: 'Of course [one] could not have imagined all the complications – they arise in the course of developing the theory and testing it' (Levins, 1982).

Instead of complex models, Levins favours simple models, that is, those with only a few elements or few relationships, e.g. two competing lizard populations. He advocates a strategy of model-building that has been popular and influential in community ecology for the last twenty years (Kingsland, ch.8). In brief, the strategy

is that biologists should use simple models to generate qualitative and general insights, and should interpret discrepancies between the model and reality to imply the need for some additional biological postulates. Qualitative insights and discrepancies together enable biologists to generate interesting questions for investigation – the primary criterion, for Levins, of a model's usefulness.

There are problems, however, with such a strategy of model-building. In practice, few community-level properties have emerged. In recent years both the generality and the usefulness of ecological theory drawn from simple models has been vigorously questioned (Strong *et al.*). The leaders of the reaction construct 'random' models which they claim fit as well as the 'deterministic' models of Levins and others. In chapter 6 L&L wield dialectics to cut down the arguments and to reject the categories of the random modellers. L&L's counterreaction notwithstanding, many issues about the use of simple models in a dialectical ecology cannot be cleared away by counterposing dialectical determination against randomness or by counterposing dialectical theory against the 'stamp-collecting' of the anti-generalists. The discussion that follows should convey that these issues are difficult ones, and should reinforce the need to challenge the social construction of what is known and knowable.

Simple models give priority to a particular hierarchical view of complexity – namely, that complexity is nearly decomposable. The mathematical analysis of simple models requires this assumption, even though it is often overlooked. The use of simple models assumes that a model represents a subsystem that is effectively independent of its context – by virtue of its separate time-scale or location in space, or because the external interactions are weak compared with the interactions within the subsystem. The problematic implications of this assumption are perhaps clearer in the social realm: The emphasis on systems that are decomposable (in contrast to richly interconnected systems[10]) is the starting point for Herbert Simon, theorist of economic rationality, and all others who want to manage complex situations and maintain their stability, e.g. by dispersing volatile inner-city concentrations into separate suburban 'subsystems'.

Simple models also give priority to ideal forms (perfect crystals,

deep structures and so on) on top of which the actual world is some imperfect performance. Dell Hymes, in the context of anthropology and linguistics, reinterprets the equivalent of Levins's strategy of modelling as 'simpling':

> Like sampling, 'simpling' is a technique for reducing the complexity of reality to manageable size. Unlike sampling, 'simpling' does not keep in view the relation between its own scope and the scope of the reality with which its deals...It then secures a sense of progress by progressively readmitting what it has first denied. 'Simpling'...is unfortunately easily confused with genuine simplification by valid generalization.

The issue Hymes has identified is familiar to many who want to preserve the local and historical contingency of their subject matter, and who cannot (or have no desire to) reduce the contingency through controlled experiments on isolated systems. How then can you generalize without using simple models and without reifying their abstract categories, yet without dissolving into particularist compilation of case histories? Again biology might look to these other fields for suggestions. In his history of societies interconnected by European expansion since the 1400's, Eric Wolf derives generality out of the incredible complexity, connectedness and change he surveys (1982). Wolf does this by organizing the particular detail around interpretative themes, for example, a Marxist conception of modes of production, or more precisely, the dynamic of interplay between intersecting modes of production.

Wolf also calls attention to an orientation that is common to most inquiries into complex situations. As a generalization the focus is on recognizable structures – e.g. in ecology populations, communities, ecosystems... – and how those structures are maintained. We could, alternatively, view structures as problematic, as 'intersections in particular time and space of describable processes which involve...change' (Vogt), in other words, as 'temporary accommodations to...the forces generating the processes' (Wolf, 1974, p.65). L&L clearly sympathize with such a dynamic view of phenomena: 'Parts and wholes evolve in consequence of their relationship and the relationship itself evolves' (p.3). But their dialectics preserves the concept of bounded system within which internal contradictions motor change. Wolf's approach, however,

by making fluid the boundaries and internal composition of structures, calls the unity of structures into question; the internal and external relations, structured and contingent, generate change.

Once we accept that complexity might not necessarily be divisible into systems or unities, then we should also question the central metaphor of mathematical modelling. In what sense, after all, can we think that nature is like a mathematical model? It is not that nature is like a set of algebraic equations. A mathematical model is a system whose behaviour is determined by components with predefined properties. So is a machine; it is a mechanical view of nature that mathematical models facilitate. If nature is like a machine, it is also very unlike one; organisms are capable of novel and flexible responses to situations not previously encountered by them (including changing human interventions), and enter in arrangements that can confound proposed system boundaries.

Levins has commented that 'all models are lies' (1982) – provisional representations which we should not entirely accept. This is a healthy, if somewhat vague, attitude to any given model. But the issues I have raised concern the very approach of representing nature with models. An ecology of complexity needs a more critical, impatient strategy than is implied by Levins's waiting for simple models to 'outlive their usefulness'. Ecology poses a huge challenge to conventional scientific ways of knowing because, in order to be able to theorize about simultaneous structure and change, ecology requires a reduction in the distance between theory and action. To assess the local and contingent and keep track of the shifting boundaries and composition of ecological 'systems' we need, I believe, to be engaged with the situation we are 'modelling'. A brief account of one modelling enterprise should back this contention.

After three weeks in West Africa and a year back at MIT building his system dynamics model of nomadic pastoralists, Picardi declared that drought and desertification in the Sahel were the inevitable 'tragedy of the commons' – individual self-interest leading to destruction of the common resource. History, although complicated, tells almost the opposite story. Range-management schemes that individualized rights of access to pasture, not to mention most other development interventions, have failed and often been disastrous. Herders, in contrast, have shown time and again their ability

to experiment and innovate in response to changes in their social-economic-ecological setting. Their crises – famines and overgrazing – stem mostly from their key ecological strategy, mobility, being constrained by the imposition of national borders, the expansion of agriculture, and range-management schemes, which substitute the judgement of persons remote from the scene for the microecological sensitivity of the herders (Brokensha *et al.*). In the crises and resistance of these 'ecologists' we might find models for engagement with our own changing socioecological complexity.

Pragmatism vs 'Science Criticism'

To develop better accounts of complexity in both ecology and evolutionary biology, I have been suggesting that we need to entertain the possibility – even the necessity – of different practices. But L&L's work raises questions about the production of science that are more general then those about their project of dialectical ecology and evolutionary biology. How can scientists work out their critical orientation and political commitment within the science of the Scientifically Dominant World and yet not become marginal or isolated? Even more generally, why does the intellectual work of radical intellectuals so rarely draw them into transformative political activity?

When L&L discuss agriculture and public health their views about practice are clear and explicitly anti-capitalist: they prescribe a move outside US science. On the other hand, their critique of evolutionary biology and ecology consists primarily of a dialectical perspective, without much attention to practice. The moderateness of this critique makes more sense if we recognize that their actual work in these areas is integrated into the social structure of US science. That structure provides the practical basis, intellectual exchange and continuing support for their work. But we should also note some of its other features: discoveries about a technically manipulable 'nature' are afforded a privileged status. The structure is beyond the individual to change; at the same time it preserves a distance between intellectuals and any transformative, collective struggle that might undermine these conditions of existence for intellectuals. So, although L&L describe their work as self-

consciously dialectical (p.viii), it also shows a pragmatic accommodation to both the constraints and the facilitation that accompany their institutional setting.

It is difficult to run counter-current in science. Furthermore, no individual through some act of will can escape their context. L&L realize that, notwithstanding their conscious commitment to dialectics, their work also reflects, in their words, the 'mechanistic, reductionist, and positivist ideology that dominated our academic education and that pervades our intellectual environment' (p.267). Lewontin's research work, which I described earlier, illustrates this. Instead of being chosen for its dialectical merit, this work looks much more like a research programme Lewontin inherited from his teacher, Dobzhansky, who defined evolution as a change in the genetic composition in populations and believed that the existence of genetic diversity was essential for progress and pluralism – a liberal form of biological determinism. Although Lewontin did not embrace Dobzhansky's political motivation, he has pursued the research programme, with a deep investment in knowing about the world, a keen sense of what 'problems [would] yield to attack' (p.3), and with few difficulties in securing recurrent funding and prestigious appointments.

I am pointing to L&L's pragmatism, while earlier I drew attention to the idealism of their dialectical prescriptions. But there is no paradox here; or if there is, it is one of the larger political-intellectual tradition in the USA, in which idealism and pragmatism are very happily married. The same people who appeal to individual rights and freedom, to equal opportunity and justice – all supposedly guaranteed by their constitutional democracy – find meaning in achieved results, and accept that those with power to act both set the terms and circumscribe the possible. Missing, squeezed out from between ideals and action, is analysis of the structured power relations that limit or facilitate social action.

Now I do not mean to imply that L&L are uncritical of US political discourse; that they are should be obvious from their scientific partisanship. Nevertheless, both the union of pragmatism and idealism, and the lack of analysis of structured constraints on action, are part of the conditions of existence of intellectuals in the USA. L&L cannot escape or transform those conditions by declar-

ing their choice of sides. In fact, by deflecting radical science to revolutionary and Third World societies and by having little to say about change in the Scientifically Dominant World, L&L confirm that those conditions have a strong hold on their thinking and their practice.

In their chapter on 'The Commoditization of Science' L&L describe the conditions of production of science in the USA. Yet the themes marked by their subheads – research as business investment, scientists as scientific manpower, elitism, even pragmatism – seem to exist 'out there'. Connecting those themes to an analysis of their own scientific imagination, activity and production is a disappointingly undeveloped area of L&L's work. Just as their critiques of evolutionary biology and ecology leave too many of the current practices unexamined, L&L do not tease open their own experience, one of making sense of the world through science, thus affording them security and influence. These conditions of existence as scientists seem almost an unavoidable given. For example, Lewontin gives Realpolitik advice to other would-be radical scientists: find a substantive base within science so that you can both develop the insight and be endowed the legitimacy to carry out the more important work about the relationship between science and society – the path he took.

L&L might argue that the discussion of their individual paths is a diversion; because no individual can change science, we need collective struggle to subvert the social structure of science. But this almost relieves the would-be radical scientist of individual responsibility to reflect on and contest those conditions of production of the science that constitute their base in the Scientifically Dominant World. L&L mention themes of the last decade such as the separation of feeling from thinking. Yet I think it is fair to say that they work with, in effect, an Old Left model: good politics for a scientist is to do good science. And to do good science requires making good use of the opportunities afforded the scientist within the current social structure of science. We are left with an image of would-be radical scientists standing firmly on the side of the revolution but waiting for larger economic forces, the internal contradictions of capitalism, class struggle, and so on – in short, for something other than themselves to bring it about.

Associated with this Old Left model of science and politics there is an unresolved tension between scientific rationalism and the contingency of voluntary, transformative action. In fact, that tension is almost built into the project of dialectics from Engels on.[11] For Old Left scientists to hold that economic forces are the prime movers and the ultimate arbiters of social action is to transfer into the political realm their reliance on nature, as exposed through their science, to resolve disputes (in due course) in favour of the correct explanation of reality. Both positions – political and scientific – have the effect of insulating from scrutiny those scientists' basis for security and potency in the social world, namely, their making sense of the world through technical manipulations and in abstract representations. To the extent that society facilitates this technical, scientific knowing, with finance, institutions, status, etc., there is little in the experience of scientists that motivates them to yield to a more contingent, relativistic framework in which scientific work is analysed as a highly mediated social knowing of nature. And so it is not surprising when Old Left scientists, unwilling or unable to analyse their own research programmes in terms of reproducing the very social relations to which they are 'opposed', show some ambivalence or confusion about their role in transformative struggle.

In L&L's writing the tension between rationalism and voluntarism is evident. Wearing revolutionary hats, they appeal to practical action to obviate the need for an account of human nature, either as it is now or would be in some truly human society. For L&L it is enough to begin with real struggles against poverty and oppression, a negation of deeply felt suffering and a conviction that the system is basically unjust (ch.13). But then they slip from this position and make assertions about ontology, about the true nature of the world, e.g. that all objects are internally heterogeneous. Surely for most practical action the heterogeneity of atomic particles is irrelevant (though, admittedly, nuclear weapons are a terrifying exception). Yet access to some 'true' state of nature does seem important to L&L, and they are reluctant to engage in either scientific or political issues for which the access route is unclear.

Similarly, L&L steer away from considerations of transformative action when they talk about history. They make those broad historical correlations that almost override any analysis of the historically

contingent actions of people; for example they observe that the origin and subsequent changes in evolutionary theory correspond to the rise and later consolidation of the bourgeois revolution. Unless those correlations are analysed further, historical developments appear to have been, in retrospect, almost inevitable. Without analysis of how people make history there is little transformative moment left in the dialectical project. As a less obvious but related tension, L&L acknowledge that change arises both from internal and from external relations (p.227), yet their theory is often mathematical. As such it is a theory of systems, which, as I commented earlier, tends to collapse contingent relations into mechanical representations.

In contrast to pragmatic accommodation and Old Left scientism, a newer Left might follow Bob Young's rallying call – a call for greater self-reflexivity, a call to contest the social relations of science and to rely less on knowing the truth about nature as a basis for political influence: 'It is time that our theories and our [practices] expressed struggle towards socialism and prefigured that social order in the process' (1977). But if the Old Left model minimized the problem of reconciling politics and mainstream scientific interests, this new Left has made the task almost insurmountable. It is not easy to acknowledge *RSJ*'s critique of science and social relations yet still practise science; indeed, there are scientists who have moved away from the radical science orbit because of that conflict. *RSJ*'s shift into psychoanalytic discourse (see their offshoot journal *Free Associations*) is a testament to their own growing awareness that 'prefigurative struggle towards socialism' is not simply a matter of 'getting our priorities right'. 'Burning bridges' is as likely to render one isolated as a critical intellectual as it is to spawn collaborative, transformative projects (Young, 1977). There is a danger that such self-induced distance from actual material practices can feed a narcissistic envy of those whose work is facilitated – as self-limiting as the alternative, pragmatic path (see Robinson).

Notwithstanding the difficulties and diversions, I want to affirm a role for individual self-reflexivity and, as a complement, a role for human biography, in a culture of vigorous 'science criticism'. The individual is, far from being the simplest element of the social, a complex synthesis of social elements: subordination, domination,

location, fantasy, tools, imagination, labour, market, income, nutrition, gender, sexuality, voice, symbol... To explore each synthesis, both for oneself and for others, is to explore both the blockages and the supports for imagination and creation of subversive possibilities; such reflexivity is a prerequisite for scientists to see what else could be in the areas of their subjects, concepts, research practices and institutions. Although this culture of criticism is, at the moment, marginal and undeveloped I see it as a precondition for radical change in the character of science-in-society.

Against accusations of 'individualizing' or 'psychologizing' social issues, I would claim that the exploration of the 'subversive potential of human subjectivity' (Robinson) is indispensable to would-be radical scientists, who have a special responsibility for social transformation in the Scientifically Dominant World. But, because of scientists' privileged positions, or even simply because of the privileged status of science in society, it will rarely be the case that experience of alienation as scientists by itself provides the direction and drive for their struggle. Unmoved by obvious necessity, most radical scientists have to find other sources of desire to struggle for change – desire in likely contradiction to their other desires, e.g. for a secure career. That is, desired political change might undermine the very conditions that produce their power to move in the material world.

L&L's dialectical project is limited by their deflection of the difficult issues of transformative practice in the Scientifically Dominant World. I may have created the impression that the limitations of their attempt is a personal failing. It is true that they could have, for example, linked into European debates and had their dialectics subject to critical commentary. Yet this speaks to a social fact more than an individual failing – namely, that there are, or have been, analytic debates about science and politics in Europe, whereas pragmatism-idealism saturates the discourse in the United States. And working at Harvard they are on 'high ground', above any currents that might draw them into collaborations, into engagements that might render realizable what seems mere fantasy under the very real hegemony of pragmatism.

I expect that many readers of *The Dialectical Biologist* will have directed their eyes at the high ground and will be focusing on

whether L&L convince them that scientific theories should be or can be dialectical. These readers' experience of the world from inside the academies will provide few sources of practical response to the more radical moment of L&L's dialectical project, that is, the challenge of informing science with social critique. Nevertheless, I hope Levins and Lewontin are not diverted into proving that their work really is dialectical. Instead they should, with other radical scientists, enter into, and allow their work to be subject to, a culture of 'science criticism'. Against pragmatism we should bring self-reflexivity and social criticism into the heart of the production of our science.

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Notes

1. The paper reprinted as chapter 7 disputes conventional interpretations of the impact of Lysenkoism on Soviet agriculture. Bob Young (1978) continues the discussion, making criticisms of L&L's use of dialectics for reasons somewhat similar to those expressed in this review.
2. The entries in Bottomore's Dictionary for 'dialectical materialism' and 'dialectics' give an excellent summary of these debates. How some of the issues have affected Marxist interpretations of Darwin is discussed by Paul (1979). Levins does take up the debates allusively on p.227 and explicitly in his 1982 paper where he criticizes Colletti, who rejected a dialectics of science while accepting dialectics as part of a project of materialist sociology.
3. The sections on evolution and adaptation reprinted in the original English from the *Enciclopedia Einaudi* are good arguments for an English translation of the entire encyclopaedia. My craving for this was reinforced when I happened to see the diagrams of conceptual connections displayed in the index volume of the *Enciclopedia*.
4. Lewontin's response to Jensen is reprinted in Block and Dworkin's anthology on the IQ controversy in the USA.
5. Just what societies qualify as revolutionary is a tricky point. Cuba, Nicaragua and Zimbabwe, for example, are not without their capitalist and centralist tendencies. Contradictions can be overlooked, but not so easily avoided.
6. For a statement and discussion of Marx's view of determination see *Grundrisse*, p.100ff. in the Vintage reprint.

7. Although L&L identify the Cartesian world-view as the culprit, it might be more appropriate to point the finger at the Newtonian practice of analysis into mechanical units and at Kantian philosophy, which enshrines this practice in *a priori* categories of space, time and causality.
8. I find Foucault's 'eventalization' suggestive but it is not clear from his history whether this was the method he actually used.
9. The animal world proceeds without history, rendering the subjectivity of non-humans quite different from that of humans.
10. For a contrasting theoretical account of how richly interconnected complexity can arise by development over time, see Tregonning and Roberts.
11. L&L dedicate their book to 'Frederick Engels, who got it wrong a lot of the time but who got it right where it counted'. What strikes me as more interesting than Engel's correctness or not are the ambiguities and tensions that Engel's dialectical explanations of society and nature exhibit. Despite the nineteenth-century flavour of his writings, Engels provided a checklist, in effect, of issues that still need to be addressed by any modern dialectician (or student of complexity), probably also by every theoretician committed to transformative action. Do we mean dialectics to be about laws, or is it about interpretation? Are these laws or interpretations applicable to both nature (science) and society (political theory)? Is dialectical change born of law-like necessity, determined by the internal relations of some unity or 'system', or are contingent, 'external' relations crucial for change? Is change a progression, an unrolling of something already implicit (communism after capitalism, the embryo after the egg), or a rough-and-tumble of construction, reconstruction and dismantling of social/natural material by particular human/biological agents? Is there a dialectical *method* by which we derive by analysis and resynthesis the true determinations of reality, or is a correct account of 'reality' relatively unimportant for those engaged in everyday transformative practice? Is the dialectician a philosopher at a distance from action, or a revolutionary?

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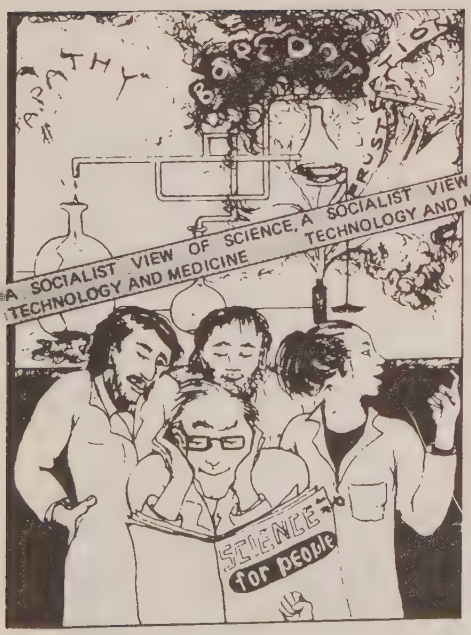

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NUCLEAR POLITICS IN YUGOSLAVIA

Les Levidow

'Jederska Energija? Ne, Hvala!' Or, if you don't speak Slovenian, 'Nuclear Power? No Thanks!' Yes, the familiar anti-nuclear badge has appeared in yet another language, the one spoken by the two million people of Yugoslavia's most European, affluent and industrialized Republic. Can we expect an anti-nuclear movement there?

Such a prospect seemed as unlikely in another European country, Ireland, when the government announced a nuclear power programme there in 1977. Yet Ireland's movement proved itself the most successful in Europe. After a few years of mounting obstruction and argument, it saw the government simply abandon its nuclear plans.

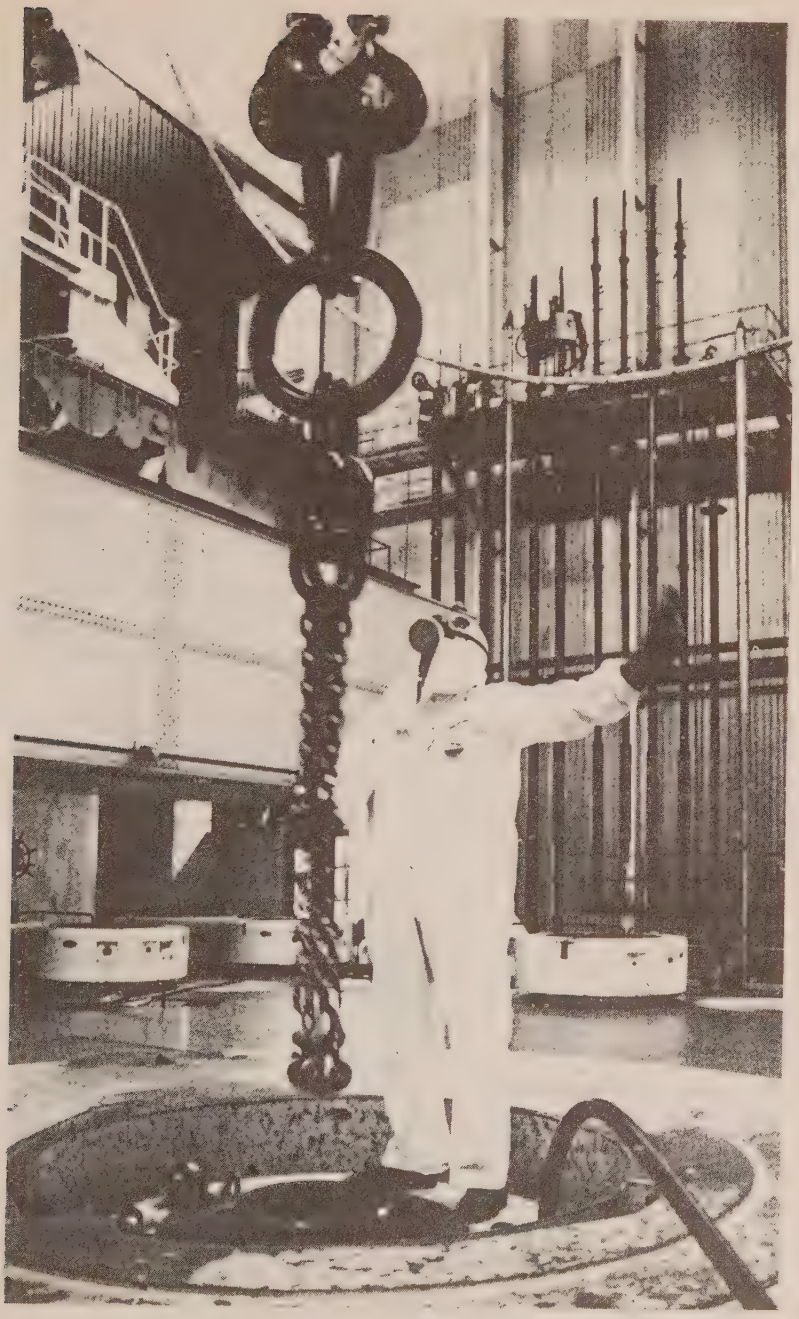
Now in Yugoslavia we can see the beginnings of an anti-nuclear movement there as well. The strongest sign came after the Chernobyl radioactive cloud passed over the country. On 10 May about two thousand people attended a demonstration, called by the Independent Peace and Ecological Movement. Marching through the main street of Llubljana, the Slovenian capital, they raised slogans such as 'Use Brains, Not the Atom' and 'Stop the Nuclear Disaster'. The demonstration made several demands on the government:

– that it immediately protest the Soviet government's secrecy over the Chernobyl disaster and demand information on the damage caused by the radiation;

– that it hold a referendum on Yugoslavia's own nuclear programme;

– that it close down Yugoslavia's only uranium mine (in Slovenia); and

– that it form an independent commission for regulating all radioactivity sources in the country.



Since the demonstration, anti-nuclear opposition has been growing especially in Slovenia and Croatia, whose town of Prevlaka would be the most likely site for Yugoslavia's next nuclear reactor.

As a result of this opposition, the federal government has announced a delay of at least four years in ordering another reactor, while still leaving its options open.

Even Before Chernobyl

Chernobyl crystallized an anti-nuclear sentiment that had been growing long before that disaster struck. Although Yugoslavia has had a nuclear plant in operation at Krško (in Slovenia) for several years, the nuclear issue was reopened last year when the President announced plans to build four more by the end of the century. In response, Slovenian delegates to the National Assembly proposed a five-year moratorium on nuclear expansion, for financial and economic reasons.

The proposed moratorium reflected growing anti-nuclear attitudes in the country's only political party, the League of Communists, as well as in other organizations. When the Alliance of Socialist Youth of Slovenia held its annual congress in April – symbolically, at Krško – it voted to oppose any further nuclear development. Their counterpart organizations in Serbia and Montenegro did likewise. And when the Socialist Alliance of the Working People of Yugoslavia held a conference organized with a pro-nuclear bias, the majority of delegates opposed nuclear power!

That pre-Chernobyl opposition came partly for economic reasons. Like Ireland, Yugoslavia ranks high in the foreign debt league table – $20 billion, or nearly $1 thousand per person. The IMF has generously offered to help the country increase that figure in order to expand its nuclear programme – though of course not for any other expenditure. The IMF is eager to help prospective customers to choose among the dozen or so nuclear firms desperately competing to make good on an investment they can no longer recoup through further sales in their own countries (see Spence).

For many other countries shopping for nuclear technology, its weapons potential seems a major consideration, but not for Yugoslavia. A generation ago, a group within the ruling elite did have

ambitions for nuclear weapons. The group was part of the 'bureaucratic-collectivist tendency', which attempted to maintain autocratic state control over the economy, with damaging effects. That tendency became discredited and finally lost out in the party's 1965 turn towards a new approach: market socialism and workers' self-management. The new approach, which inflicted its own damage, turned out to mean largely management of profit maximization by professional and skilled workers (see Further Reading). But in any case, that was the end of Yugoslavia's military motives for nuclear imports.

Forces For and Against

Who in Yugoslavia, then, wants more nuclear power? As in other European countries, prime supporters are the electricity-generating companies, such as the Slovenian General Power Company. It has a major pro-nuclear ally in heavy industry, which benefits from the low electricity rates that the Company grants it in preference to the smaller-scale consumers. Heavy industry, especially steel, is counting upon continued low rates in return for its political loyalty, even if nuclear power makes electricity generation more expensive for everyone else. Support also comes from the scientific and engineering professions, who anticipate enhanced prestige and career opportunities through nuclear power. Of the country's six Republics, only the Croatian government has so far supported the nuclear expansion proposal; apparently it hopes to get the next plant, since the original Westinghouse contract specified two plants, the second of which was to be sited in Croatia.

The country's first plant was built in Slovenia at Krško, a small town between Llubljana and Zagreb. The Krško plant has remained in so much secrecy that its operation has provided little basis for anti-nuclear arguments. What is known is that the Westinghouse contract required Slovenia to buy not only the physical plant but also the management structure, though the plant is staffed by Yugoslavs.

Perhaps partly for that reason, the anti-nuclear forces include leading computer firms, which can see that they will be excluded from any high-tech developments involved. Also opposed are many

smaller industries, which do not get the concessionary rates for electricity.

Many leading opponents are social scientists, economists and environmentalists. Some of these define themselves as 'Greens' yet hold political influence as party members. Another anti-nuclear sector, peculiar to Yugoslavia, is the 'Self-Management Society', an organization of factory workers' groups aware that nuclear power precludes any kind of workers' control. Among people more generally, anti-nuclear suspicions grew when secret documents from the Philippines revealed that Marcos had received payoffs from Westinghouse in return for its nuclear contracts there; afterwards, a Yugoslav journalist made a similar accusation about his country's politicians, who have not denied it.

Nuclear Arguments

The earliest signs of anti-nuclear sentiment appeared in Slovenia. In its capital, Llubljana, over six hundred people attended the country's first-ever public meeting on nuclear power, held on 27 March. It was organized by *Časopis za Kritiko Znanosti*, a long-established Marxist journal for the critique of science, and was financially supported by the Alliance of Socialist Youth of Slovenia.

Both the journal and the Alliance, which have party members, receive Slovenian state subsidy. Thus, through their own politically independent organizations, 'ultra-Left' Marxists have been able to use state funding and even the party to oppose Federal government policies – for example, in trying to establish a legal right to strike, as well as in opposing nuclear power.

The 27 March public meeting, lasting five hours, revealed little about nuclear power in Yugoslavia but much about the country's political tensions. The meeting opened with two hours of presentations by five invited speakers, four of them from abroad. The only Yugoslav speaker was a physicist, Miha Tomšič, who argued that a small investment could save the 30 per cent of energy consumption currently wasted by industry, that the country's national grid loses through transmission fully one-third of all the energy it handles, and that the country has realized only a fraction of its potential hydroelectric power. Another invited physicist, Dragan Jovanović, could

not attend because he had received instructions to go on a business trip coinciding with the public meeting – a coincidence that raised further suspicions. The four foreign speakers presented anti-nuclear arguments from their research into nuclear security and the nuclear export market, and from their experience in opposing nuclear power in Britain, Austria and West Germany.

The ensuing three-hour discussion, in which microphones were literally passed around the audience, focused less on anti-nuclear arguments than on procedural ones: How should Yugoslavia decide? There were arguments for a petition or a public inquiry, arguments for and against a national referendum, and arguments about the futility of delegate decision-making. There were suspicions that politicians would place the interests of their particular Republic above those of the people as a whole, or that each firm would use the issue to advance its own narrow interests. With the nuclear issue, as with all economic development issues, aspirations for democratic decision-making founder upon the contradictions of a 'market socialism' which has meant throwing firms into competition with each other and with foreign imports, thus encouraging profit-maximization and inter-Republic rivalry for scarce infrastructural funds.

At the public meeting, no Yugoslavs made their arguments in class terms. Anti-nuclear speakers spoke mainly in terms of conservation or ecology. One pro-nuclear speaker argued the necessity of increased energy supply for increased labour productivity. What would that mean? Since the country's decisive turn towards market socialism in 1965, increasing labour productivity has meant mechanizing and automating production, displacing labour, thus increasing unemployment and labour migration, especially to West Germany. (Ireland has suffered similar effects as the government implemented the 1958 Whitaker Report: tariff barriers were reduced to promote free trade, while foreign investment was encouraged by tax incentives, subsidies and grants, especially in the Shannon Development area.)

Like many Third World countries, Yugoslavia has suffered not only through increasing unemployment (now 30 per cent) but also through exporting the unemployed, especially from the country's less industrialized areas in the south. Yugoslavia is not directly

dominated by the multinationals to the same degree as Third World countries (or Ireland), but an analogous imperialist exploitation operates through Western bank loans handled by Yugoslavia's central bank, which of course gives preference to profitable firms. In the name of energy independence and economic progress, nuclear expansion would aggravate the country's dependence, unemployment and foreign debt.

As the only member of the Non-Aligned Group in Europe and a self-proclaimed socialist country, Yugoslavia may seem like an anomaly. Yet the nuclear issue there reveals much about the role of the nuclear export market as a whole, as nuclear firms compete to offer the best financial terms, supplemented by the IMF offer. It remains to be seen whether the initial anti-nuclear rumblings there (and elsewhere) become strong enough to stop nuclear export once and for all.

Further Reading

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HAZARDS FOR EXPORT: DOUBLE STANDARDS?

T. Jones

Essay review of Jane Ives, ed., *The Export of Hazard: Transnational Corporations and Environmental Control Issues*, Boston, Routledge & Kegan Paul, 1985, 229 pages, £14.95 hb

In the decades immediately after the Second World War, chemical technology became the cutting edge of capitalist development. Within this period industrial and agricultural production became heavily dependent on the use of highly complex synthetic chemicals. The massive growth in chemical production, including the development of new industries such as plastics and synthetic textiles, was accompanied by a horizontal move by the chemicals industry into sectors of production as divergent as agriculture, textiles and packaging. (For background and figures see Alliance for Safety & Health, 1982.)

The introduction of this chemical technology and its related work processes was a great leap forward for capital in terms of profitability, productivity and removing control over work processes from the hands of workers, but it proved to be a leap into the abyss for the rest of us. The massive spread of chemical use has resulted in an increased exposure to toxic chemicals through all stages of what can be described as the toxic cycle, though it is no more a cycle than the 'nuclear cycle'. This exposure occurs during mining/extraction, refining, production, transportation, consumption and waste disposal.

Thus the toxic nature of its work processes and products has become the most important characteristic of certain sectors of production. Capital earned massive profits in these sectors by ignoring the social costs of toxic production and consumption – costs which were borne not only by workers at the point of production but also

by the general population. 'For it is apparent that the "squandering of human lives" does not only occur *within* the gates of the nuclear plant or chemical factory but is as "social" as the labour that produces the radioactive electricity and poisons' (Midnight Notes, 1980).

This toxic development did not proceed unchallenged. Successful struggles against exposure to toxic industry were undertaken both in the workplace and in the general environment. This refusal by both workers and the general community to bear the costs of toxic production led to state intervention and regulation of toxic industry in the major metropolitan countries. This regulation resulted in major increases in capital costs for both health and safety and environmental control measures for the chemical process industries: by 1979 the chemical and allied process industries expected to devote 15 per cent of capital spending to satisfy government health and pollution regulations. (Whether this regulation was successful in protecting public health is a separate issue that will be addressed below.)

Similar regulations to control consumer product safety were also introduced. During the 1970's increasing numbers of products – pesticides, drugs, foods, even pyjamas – were banned in the USA, thus presenting the producing firms with a major crisis in obtaining any return from their investment in these products. US capital responded in two ways: by legal challenges to regulations and by exporting the crisis. Thus the Dalkon Shield, tris-treated pyjamas and pesticides such as Phosvel were dumped on peripheral countries with little or no regulation. If toxic products could be dumped, then why not dump toxic production as well?

In 1979 Barry Castleman applied the hazard export analysis to industrial location. In a classic paper (which unfortunately is not included in the volume under review), he argued that the increasing costs of government regulation, primarily in the US but also in Japan and Europe, would lead to a major flight by toxic industry from the regulating metropolitan countries to the unregulated peripheral countries:

> Banning of unsafe consumer products, foods, drugs and pesticides here has often led to the subsequent export of these products. US pollution control laws and occupational health standards may soon lead to *a wholesale exodus in major industries*, as manufacturers move overseas

> to avoid the large costs imposed here while continuing to sell their products in the US...In many of our most polluting and hazardous industries, existing plants are very old and incapable of being made safe by adding on control devices. These plants need to be redesigned and rebuilt, not just fitted with exhaust fans. Faced with this reality, some manufacturers find it economically attractive to move hazardous plants to less restrictive localities rather than to stay where they are and meet tough regulations. Today, 'runaway hazardous shops' are leaving the United States, whereas in the past they only crossed State boundaries within the country...The economy of hazard export is emerging as a *driving force in new plant investment* in many hazardous and polluting industries. Poverty and ignorance make communities in many parts of the world quite vulnerable to the exploitation explicit in hazard export. (Castleman, 1979, emphasis added)

That position has become the predominant critical approach to toxic industry in peripheral countries. Ives's collection represents the latest and heftiest manifestation of that analysis. Here I will use the book to suggest that the approach has major theoretical and political flaws.

No Wholesale Exodus

I should like to begin with a concrete example. In both her contributions to this book, Ives cites a major pollution controversy relating to the operations of Merck, Sharp & Dohme, a Bermuda-registered subsidiary of the US pharmaceutical multinational Merck & Co., Inc., in Ballydine, Co. Tipperary. There botanical pollution, a large number of cattle deaths and fertility problems, as well as human health problems, have all been linked to chemical emissions from the plant, particularly to emissions from its toxic waste incinerator. (For background on this see Merck, Sharp & Dohme Study Group, 1983. For information on Merck's double standards, see Jones, 1985.)

While Merck, Sharp & Dohme certainly operated its toxic waste incinerator below applicable US standards, Merck did not site this major pharmaceutical plant in Ireland because of the lack of environmental protection and occupational health and safety regulation, but mainly because of the tax advantages that production in Ireland conferred on the corporation. This analysis is supported by

Before and After
In March 1983 a tractor on John Hanrahan's farm takes away dead animals for burial. Hanrahan claims that the deaths are due to pollution from the Merck Sharpe & Dohme plant in nearby Ballydine. Under pressure from farmers, the government installs an air monitoring device on his farm. Shortly (June 1983) the government displays its 'sensitivity' by removing the device, after a report calling for further study is interpreted by the local county council as exonerating the plant.

Opposite: Local farmers protest removal of the device. A year later, livestock continue to die.

1458

a US trade union, which cites Ballydine as Merck's main tax haven (OCAW, 1984). Nevertheless, the lack of such regulation provided the icing on the cake for Merck. The savings on pollution and safety measures in locating this plant in an underregulated peripheral country like Ireland (even if they had been as high as the 30 per cent of capital investment sometimes cited) are incidental savings to a corporation operating in a country where the average annual return on US capital is 29 per cent and, in Merck's case, where the company estimates its return to be $1 million a week in tax-free profits on an original investment of $60m. The classic hazard export analysis obviously does not apply here: the importance that Castleman's original article attributed to regulation as a determining factor in industrial siting is simply not convincing.

Reasons for the failure of Castleman's original analysis are not hard to find. Castleman was drastically over-optimistic about the continued success of regulation. His misjudgement was due partly to his reliance on the asbestos experience: no other US toxic industry has suffered as great a defeat as the asbestos industry did. (It should also be noted this defeat was the culmination of a fifty-year struggle.) Furthermore capital's counterattack on state regulation in the US was to be highly successful, particularly in forcing regulators to consider economic costs to capital in their decisions, finally leading to the deregulatory spree under the Reagan regime, which was set back only by the tendency of Reagan's deregulators to shoot themselves in the foot.

Castleman's original paper was quickly taken to task by Levenstein and Eller (in this volume); they argue that no drastic increase in US investment abroad has followed the establishment of the Occupational Safety & Health Agency (OSHA), while the US continues to be a sufficiently attractive environment to draw foreign capital. Levenstein and Eller fail to realize that the capital flight Castleman predicts is selective rather than wholesale. As Castleman notes in his response, 'a flyspeck in the cashflows of international investment can nonetheless be a public health disaster'.

Castleman responds to the Levenstein and Eller critique by changing his position: 'Whether there is a *wholesale* exodus of hazardous industries fleeing regulation and liability is beside the point. Whether the industrial planners tell interviewers that environmen-

tal regulation is a *main* determinant is also beside the point. The point is that there is international trade in industrial hazards, increasingly so, and that this has predictably grave public health consequences' (pp.91-2).

Thus in his second major survey of the problem (reprinted in this volume) Castleman withdraws from his previous position and falls back on the concept of double standards, an analysis which does not entail major flights of capital offshore: 'An integral part of the hazard export cycle is an international double standard in industrial hygiene and pollution control. With the industrialization of the Third World, clear-cut examples of hazard export may be far less numerous in the future than simple cases of the double standard' (p.62). Obviously it is possible to argue that such flights as would occur would be in specific areas of industry.

Levenstein and Eller's other arguments against Castleman's thesis are unsatisfactory. Their argument that capital has the technical ability to adopt profitably to regulation, using the vinyl chloride example, ignores the fact that capital won the vinyl chloride battle by succeeding in getting a standard, rather than a zero detectable level, imposed. They also ignore the way capital deals with this regulation, by emitting the regulated toxic chemicals into the general environment: this simply transfers and disperses the problem. Nor does regulation have to be effective to increase capital's costs.

For either Castleman or Levenstein and Eller to prove their positions would require a sectoral analysis of US direct investment abroad. Unfortunately no one seems to have undertaken such an analysis. The absence of a major geographical analysis of worldwide siting of hazardous plants in the last decade is a major drawback here. The lack of this analysis has allowed capital to dismiss the hazard export analysis by arguing that environmental costs are not the decisive factor in plant siting.

A more correct view would be to see regulatory costs as one of a group of factors affecting the location of industrial plants while, in some cases, these costs are the decisive factor. (A recent example would be the decision of Angus/Isochem to locate in Ireland rather than Wales.) This more realistic view of capital's decisions on the siting of hazardous plants makes the argument more compelling and less easily dismissed.

Castleman's later double standards position is much more tenable than his original hazard export position. Whatever the decisive factor in industrial location is in any particular case, there is no question but that capital takes wholehearted advantage of such lowered regulatory costs as are available in any location. Even apologists for industry such as New York University's Gladwin and Walter – who argued after Bhopal that 'regulations driven by nationalism may lie at the core of any in-depth explanation of Bhopal's tragedy' – admit that multinationals operate a patchwork of safety standards worldwide, which depend on local law and practice. This parallels the situation regarding information on hazards of drugs and pesticides. 'Most studies in recent years have found varying degrees of "local accommodation" rather than "global standardization" to be the prevailing approach of multinationals to environmental management as well as occupational health and safety in their overseas operations' (Gladwin and Walter, 1985). These are the same studies that toxic capital has used to dismiss the hazard export argument.

Who Is Safe?

Before going on to deal with the major political flaw in the hazard export argument it is necessary to mention a few other failures of the analysis. By focusing on the threat posed by multinational capital, it ignores the threat posed by indigenous and state toxic capital. Bhopal was the third major petrochemical accident to occur in 1984. The other two can be attributed to state capital. In November 1984 an LPG explosion at a Pemex facility in Ixuatapec, Mexico, killed at least 452 people and injured over 4,000; more than 1,000 people are still missing, many of them buried in the rubble by the Mexican state. In February 1984 at least 500 people were killed when leaking gasoline from a pipeline owned by Petrobras exploded in Cubatoa, Brazil. State capital in particular – Pemex, Petrobras, Nitrigin Eireann Teoranta, etc. – has a very bad record. Therefore it is not the form of capital that is important, but the material content of production.

Another problem is caused by the implied view that nonregulating countries which suffer hazard export are solely peripheral

nations. England's Celtic margins, for instance, have been targets for hazard export. Consider ReChem International at Bonnybridge and Pontypool, Windscale/Sellafield, as well as the NEDO-proposed hazardous chemicals park in Cumbria, while England itself has been the target for toxic waste dumping from Ireland and the Netherlands. Another corrective is provided by Weeks, the token trade unionist contributor to this volume, who points out that, just as toxic capital has migrated to peripheral countries, so have workers from peripheral countries migrated to the US to undertake toxic labour. California is the most obvious example here, with the women workers of Silicon Valley and its farmworkers being mainly migrant workers.

Furthermore, the view that hazard export results from increased regulation in metropolitan countries is more limiting than a view which sees hazard export as an inevitable consequence of the internationalization of capital and its model of industrial development. The former sees hazard export primarily in terms of specific high-risk industries such as asbestos, petrochemicals, smelting; while the latter would emphasize the spreading of toxic chemicals through industry and the environment in general. The former would be hard put to explain the ammonia leak two months after Bhopal which injured 130 workers at a prawn-processing plant in North Sumatra, while the latter would have no such problem. Elling formulates this problem extremely well:

> Also, still with respect to the need for better definition of the problem, what should be considered hazardous industry? Only those substances and conditions (such as noise) for which OSHA or some similar regulating arm of the state in another core nation has set a control standard (in the case of noise an inadequate standard, since some 18 per cent of workers are expected to go deaf over a working lifetime at 90db)? The vast processes of human degradation and exploitation, often of women and children as well as men, going on as part of the worldwide Taylorization of labour seems hazardous and ultimately intolerable and insupportable. This is a much greater, deeper problem. Yet it is not regulated. (Elling, 1982)

The latter view is supported by the major dissenting contribution to this volume: Michaels, Barrera and Gacharna's 'Occupational Health and the Economic Development of Latin America', which

starts from an analysis of actual conditions in the peripheral countries. It concludes: 'It is our belief that throughout Latin America, the importation or growth of hazardous industries, fleeing environmental regulation in their country of origin, is not the primary source of potentially disease-causing workplace hazards in Latin America.' It is possible to argue from their paper that the major attack on the health of the people of Latin America has its roots in the often forced reallocation of arable land for export-orientated agriculture, with its results of landlessness, migrant labour, malnutrition, etc. A political argument developed from this would see the whole process of 'development' – that is, the imposition of the Western model of production, both in agriculture and in industry – as an attack on the health and lives of the people being 'developed', rather than singling out particularly nasty industrial forms of that 'development'. This is a much more radical argument, from which it follows that we should be discussing not how to control technology transfer but how to end it.

The authors conclude, 'Short of major structural modifications, however, it is unlikely that conditions in most Latin American workplaces will improve markedly in the near future' Their realistic conclusion faces the real role of the state in peripheral countries: this 'development' by murder will continue unless it is contested by social force. A final point the authors make needs to be stressed: 'the poorly developed health care delivery systems of these nations ensure the existence of an occupational disease compensation problem *similar to that of the developed countries*: the work-related aetiology of most occupational diseases goes unrecognized and therefore uncompensated' (my emphasis).

Here we have the major problem with the hazard export analysis, the way in which it fosters the illusion of safety in metropolitan countries. Noble points out 'the missionary tone of much of these discussions, the hesitant but still present assumption that the situation is more under control "here" than "over there" and that what is called for is, in a sense, a spreading of the gospel.' Levenstein and Eller support this by pointing out how ineffective OSHA has actually been; this point is made more real by the only contribution in this book from someone who has had experience of toxic workplaces, the trade unionist James Weeks. This comfortable notion of

safety has been undermined by the recent safety crisis in toxic industry following Bhopal and was always reasonably suspect anyway: if there is any difference, it is only one of degree. Samuel Epstein, asked whether a Bhopal-type disaster could occur in the US, responded, 'I think what we're seeing in America is far more slow – not such large accidental occurrences, but a slow, gradual leakage with the result you have excess cancers or reproductive abnormalities.' While Ives provides an impressive list of toxic outrages in peripheral countries, it would be possible to produce as large a list of outrages in the metropolitan countries during the same period.

Bhopal Test Case

Perhaps the best way to examine the political usefulness of the hazard export/double standard analysis is in the context of Bhopal. Here Castleman and Purkayastha's detailed article is a superb example of what research into hazard export should be. Superb though it is, I would argue that it is not a satisfactory response to Bhopal. In the context of double standards and the supposed superiority of regulation in metropolitan countries, it is worth noting the comments of Michael Wright of the United Steelworkers of America. As a member of the ICFTU/ICEF mission to Bhopal, he said that, had the Bhopal plant been in the US and been visited by OSHA & EPA inspectors, 'they might have walked away without giving any citations' (administrative charges that safety regulations have been violated). This response is also to be found in the ICFTU/ICEF report on Bhopal, one of the best responses to Bhopal I have read. The key passage is as follows:

> The specific items which caused the tragedy and the specific way they came together on the night of December 2, 1984, were unique. But the underlying causes...
> – insufficient attention to safety in the process design,
> – dangerous and irresponsible operating procedures,
> – inadequate maintenance,
> – faulty equipment,
> – cutbacks in manning,
> – inadequate training,
> – management and government unresponsiveness to safety complaints,

> – the siting of potentially dangerous plants in heavily populated areas,
> – lack of information,
> – lack of disaster planning,
> are not unique. Members of the mission represent chemical unions and have visited chemical plants around the world. In our experience the factors that led to the Bhopal disaster are common. (ICFTU/ICEF, 1985)

In terms of organized political response outside India, the hazard export/double standards analysis led activists unwittingly to fall in with toxic capital's attempt to contain the safety crisis that Bhopal represented to the peripheral countries; that analysis prevented them from using Bhopal to attack toxic capital in their own countries. This response should be compared with the use the international anti-nuclear opposition made of the Three Mile Island disaster, where the body count was invisible, though the opposition was more highly organized.

I have argued elsewhere (Jones, 1987) that an analysis of Bhopal which confines itself to the massacre at Bhopal fails to recognize the severity of the crisis Bhopal posed for toxic capital. A more useful analysis would have to take up the strikingly similar, though (by pure luck) much less deadly, release of toxic chemicals at Union Carbide Corporation's (UCC) Institute, West Virginia plant. I have dealt in detail with the similarities between both 'accidents' elsewhere, so I will only summarize them quickly here. Both 'accidents' were caused by runaway reactions. In both cases the inadequate 'safety' systems were overwhelmed. UCC took hours to reveal what chemicals had leaked at Institute and downplayed their hazards. Information on the health effects of aldicarb oxime, one of the major constituents of the Institute leak, was as lacking as information on MIC; UCC failed to inform the local authorities promptly. Both 'accidents' can be traced back to decisions based on capital's values of profit-taking and cost-cutting, Regulators and local authorities in both countries assured the population that both plants were safe, while actual inspection of plants was considered unsatisfactory by the workers' trade unions.

This analysis stresses the similarities between chemical plant operation in both peripheral and metropolitan countries. It allows us to see Bhopal as a special case of the generally oppressive design

and operation of chemical process industries as a whole. This is not simply a theoretical point. It is a practical one, of immediate use in anti-toxic struggles. It is preferable to argue that toxic industry is not safe anywhere, since this position removes any basic division between struggles against toxic capital in metropolitan and peripheral countries.

State of the Debate

The book under review is a useful indication of the present state of the debate. One major problem already referred to is the lack of empirical studies. Politically it is noteworthy that there has been no spate of well-documented and popularly orientated books on the subject from the development lobby (e.g. Oxfam), as there has been for drugs (Silverman, Muller, Medawar, etc.) and pesticides (Bull, Weir, Linnear). As Elling pointed out in 1982, 'considerable empirical, theoretical and practical work remains to be done in this sphere of concern'. In 1986 the situation remains the same. Of the empirical studies in the Ives collection, the two reviews retread well-known ground; Michaels, Barrera and Gacharna present a solid academic piece of work; while Purkavastha and Castleman's case study of Bhopal is superb.

The same cannot be said of Ives's contribution on Ireland. Her article is a terribly slapdash production, with bits from newspapers (mainly forwarded to her by Irish activists) and her impressions of a 1982 conference in Cork forming the thin white slices of a sandwich whose main constituent is material lifted from *Toxic Ireland*. That report, in which I had a hand, was produced by the Alliance for Safety & Health (ASH) in 1982.[1] This is research? This is intellectual labour? In dealing with Ireland, Ives has advantages over the activists who produced the ASH report; as a respectable academic – an environmental consultant, no less – she is in a better position to obtain information from state and industry sources than the decidedly unrespectable ASH activists. Yet she has not even bothered to obtain the EEC figures for toxic waste disposal in Ireland, much less attempt a comparison between percentages of capital investment spent on environmental protection and occupational safety and health measures by US multinationals who operate facilities in Ireland and the US.

The rest of the volume has slightly better offerings: a short debate between Castleman and Levenstein and Eller; a competent technical review by SOCMA's Waldo, a bibliography which stops at 1982, and six articles on policy issues, many of which retread the same ground. One major problem with the book is its almost total reliance on metropolitan contributors: it is scandalous that a book dealing with hazard export should have only three authors (out of fifteen) and two contributions (out of fifteen) by authors from peripheral countries, and even these voices are controlled by having co-authors from metropolitan countries. Of a piece with this is Rashid Shaikh's rather pompous bibliography, which does not list a single source published in a peripheral country. Thus we lose the experience of our comrades in the other peripheral countries. While Noble at least refers to the export of arms production, even he ignores the most obvious example of this – the 'export' of nuclear arms testing, which was one of the first major examples of hazard export. Here, as in so many other areas, the military trail-blazed for capital. While all the major metropolitan countries with nuclear arms are guilty of this, the issue is not raised at all, no doubt in part due to the lip service some of these advocates pay to hazard export's deleterious effect on US foreign policy. Among the points Corning's King gives for controlling hazard export as a policy of enlightened self-interest, the third is: 'The price we pay in terms of foreign policy "good will" around the world is sure to increase as the political stakes in Latin and Central America, in Asia and in Africa grow higher' (p.10).

Furthermore, the volume focuses almost totally on US capital. Questions such as the various perspectives on and responses to hazard export in Europe and Japan are not addressed. The majority of the contributors are US academics, business and environmental consultants, with a token trade unionist thrown in. Indeed, the debate in this book can be seen as one between various factions of US capital, state and intelligentsia over the regulation of US capital. The majority of the book's authors wish to reform and restrain US capitalism and see government regulation as the way to accomplish this.

The politics involved is reflected in the revealing lengths they go to in their arguments. Some argue that hazard export can damage

US foreign policy and commercial interests by undermining consumer confidence in the quality of American commodities. Others, by their uncritical acceptance of such loaded concepts as the risk/benefit equation, ensure that their analysis remains within limits defined by capital. Ruttenberg, for example, accepts and repeats the cliche that the risk/benefit equation in peripheral countries may be different from that in metropolitan countries, yet without pausing to examine the uneven distribution of risks and benefits that is always hidden behind this supposedly neutral equation. In that vein the only omission from the book is a representative of US capital condemning the extension of US regulation abroad as cultural imperialism, as some have done previously.

Myth of State Regulation

This volume is symptomatic of the major weakness of the hazard export analysis – its uncritical approach to state regulation. Despite the continuing advocacy of regulation by many of these contributors to this volume, they have not adequately analysed the experience with regulation in their own country.[2] They have failed to learn from the US experience, which shows time and again government complicity in outrages by toxic capital. Even Wolfe's excellent empirical analysis in 1977 – a time when government regulation of toxic industry was supposed to be running riot in the US – shows that the majority of regulatory actions relating to carcinogens were forced on the US regulatory apparatus by political pressure from trade unions and consumer groups.

Indeed, a realistic analysis of regulation in the US would be to see it as a forum for inaction, projecting a politics of reassurance fraudulently presented as a measure to protect public health. The failure of regulation to protect the public health is strikingly obvious in cases as varied as toxic dumping and the regulation of hazardous air pollutants. Of course there have been victories won in regulation. But these victories resulted from political struggles in the workplace and in the general communities themselves; the state regulation aimed to co-opt, channel and regulate the struggles as much as the pollutants. Certainly where anti-toxic opposition is too weak to mount a frontal assault against toxic capital, we should in-

deed organize to shackle toxic capital with as much regulation as can be wrested out of the state. But this is a question of tactics, not strategy.

The overall failure of regulation in metropolitan countries belies the authors' missionary zeal to export regulation. There is little value in exporting 'safety' standards when the 'safety' they guarantee is of such a dubious nature. Needless to say, none of these policy pieces considers the possibility that hazard export might be successfully fought by popular action.

Just as the authors are blind to the role of the state in their own country, so also are they blind to the role the state plays in peripheral countries. The political proposals these authors make are based on their own narrow experience in the US. Their major aim is to increase state regulation of toxic capital, despite the signal failure of such regulation in the US. Thus the major practical measure suggested in this volume is that information should be provided to the importing government on the hazards of any product or production process banned or severely restricted in the US. (Noble again is the only one who suggests in passing that this information should also be provided to trade unions and consumer groups.) The naivety of this suggestion can be seen from a recent *Wall Street Journal* report from Chile: a multinational manager told the *Journal* that government health inspectors could be persuaded not to inspect a factory for the price of a lunch and a couple of beers, while a Finance Ministry official said, 'If a foreign company wants to invest in Chile, there are very few obstacles we will put in front of it. And that includes environmental risks.' Providing information to this government is a public policy solution to hazard export? While Chile is obviously an extreme example, the difficulty of prising any information from the Irish government is legendary, while the British state is not terribly forthcoming either. Because of their failure to analyse the state, these analysts fail to recognize how governments of peripheral countries act in complicity with the hazard export that requires the exploitation and contamination of their population. It is worth recalling here Bahro's description of 'development' as 'a conspiracy between the elites of North and South'.

Ives's article on Ireland provides a case study in the inability of these analysts to face the real role that the peripheral state plays in

hazard export. Consider the ASH conference in Cork on 'Hazards in the Workplace – Poisons in the Environments', which Ives herself addressed. Contrary to her claims here, it was definitely not called 'The National Public Conference on Environmental Protection and Occupational Safety and Health', and it was not intended to propose 'public policy solutions concerning the issues of environmental pollution and occupational safety and health'. It was intended as an organizing tool, an attempt to bring together various groups opposing toxic development in Ireland. As a result, it was conveniently smeared afterwards with a 'terrorist' tag: Ives presumably should have included in her list of prestigious participants at the conference those strange bringers of public policy solutions, the Red Brigades, as the *Cork Examiner* confidently assured its readers that this group had also attended the conference.

This reality of a state prepared to intervene to protect toxic capital from attack does not square with Ives's image of state regulation responding to public concern. Similarly, in her treatment of toxic waste dumping in Scribblestown, Ives reports: 'Highly toxic pesticides, solvents, trichloroethlene (*sic*), asbestos and many other lethal substances were found, documented and shown by a deputation in June 1981 to Mr Charles Haughey, and then to Taoiseach (*sic*) who ordered an immediate investigation and cessation of the dumping.' Although the mistakes are Ives's contribution, this is lifted directly from *Toxic Ireland*. The *Toxic Ireland* account continues, however: 'The dumping ceased for a short time and then resumed and still continues at the time of writing.' It is worth noting that one of the major toxic dumping contractors in Ireland is also a major supporter of Mr Haughey's party.

Similarly Ives ignores material from the ASH report on the state's use of both ordinary police and Special Branch (political police) against those opposing toxic development; this of course is not the type of state intervention that interests Ives and her colleagues. The vacuity and neutrality of Ives's conclusions on the Irish experience of hazard export have to be read to be believed. Her article ends thus:

> The promotion of industrial development must be coupled with serious consideration for government and industry sensitivity towards environmental problems which have resulted from the rapid industrialization of

> Ireland. This must be complemented with long-term strategies and initiatives for public participation and education on public policy concerns of environmental and occupational health & safety in the Irish workplace.

Any strategy that wishes to oppose toxic capital requires a realistic analysis of the state's role in supporting toxic development and a critical analysis of state regulation as a method of controlling toxic industry. Such analyses are not available in this volume, whose statist strategies ignore the realities of peripheral countries. The advocates of 'properly trained and sensitized man (*sic*) power' in this book could take lessons in political reality from activists there.

It should be obvious that strategies which confine themselves to the provision of information to the peripheral state cannot have any major effect on toxic capital's outrages. Thus as Sathyamurthy observed in relation to nuclear power in India, 'an effective anti-nuclear movement would have to mobilize those class forces capable of challenging the entire basis of India's dependent development,' (Sathyamurthy, 1984). The struggle following the Bhopal massacre confirmed this. There is a great need for a publication from activists in the peripheral countries on their struggles against toxic capital, including their analysis of state intervention and complicity. It is an indication of the poverty of this book, and of the hazard export analysis in general, that their experience is conspicuous by its absence.

Notes

1. For example, one passage from Ives's chapter on Ireland (p.117) is taken almost directly from our publication. Here is our wording:

> The Irish economy has gone through a massive process of restructuring and reorganization, corresponding to the transformation taking place on a world scale. Before this period, the Irish economy had a very weak industrial base, made up of clothing, footwear, food processing, engineering, shipbuilding and traditional textiles. The Whitaker plan for the Irish economy, implemented in 1958, was designed to throw open the economy to foreign capital. The result of this was the rapid influx of chemical, pharmaceutical and more recently electronic industries into the country, under a grant and tax-free system managed by the Industrial Development Authority. The internationalization of the Irish economy has been dominated by US multinationals, who showed a 29 per cent return on capital in the years 1974-1977. This compared to a 12.8 per cent return on US capital in

> Europe, and a 12.3 per cent return on US capital in the world economy. (*Toxic Ireland*, p.117)

I would not object so much to Ives lifting our material if she had not made such an inept job of it. She even omits the rate of return on US capital in Ireland – information that might undermine her theoretical approach. A well-documented academic analysis from an outsider would be useful politically in Ireland, but it would not be possible to circulate her chapter in Ireland except as a joke. I will pick just some examples from many. My opinion of the political role played by the bourgeois environmental group An Taisce is confirmed by Ives's revelation here that it was the Department of the Environment all along. The map that accompanies the article provides much amusement as well. Play the new game of 'spot the multinational'. Who are Syntax, Baker Travenol, Bocton Dickinson, Goalas (a little-known Indian transnational, perhaps?), Oltn & Pents Chemical? What, a friend wondered, is the NET mentioned on page 120? A chemical? A town? A peculiar form of Irish fishing? A semi-state company? None of the above? More interesting yet, who or what is Taoiseach (p.123)? And who is the mysterious Captain Garvey (p.119)? I think we should be told.

2. On occasion this failure to analyse regulation amounts to downright dishonesty. While many of the contributors laud President Carter's Executive Order on hazard exports, they omit to say that this order was issued only after Carter had lost the 1980 election to Reagan. During the election campaign Carter had kowtowed to toxic industry's demands for less regulation.

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SOCIOLOGY PULLS ITS PUNCHES

Tim Rowse

Essay review of Karin D. Knorr-Cetina and Michael Mulkay, eds, *Science Observed: Perspectives on the Social Study of Science,* Sage, 1983, 272 pages.

The recent sociology of science has developed the argument that the content of scientific knowledge is socially determined, because – contrary to a positivist view – it cannot be solely determined by 'the real'. This argument has undermined one theme in the rhetoric with which scientific personnel sometimes present themselves. It makes us sensitive to the socially contingent nature of expert pronouncements about the presence or absence of causal connections between environmental phenomena and disease, for instance. So it should now be less possible to settle a political argument by referring to an absolutely authoritative account of the state of natural phenomena. All such accounts are constructs determined partly by the shared cultural resources (background assumptions, cognitive and logical conventions) of the social actors making the claims. Just as a study of labour processes as objects of class struggle can de-reify technology, so the sociology of science helps in the de-reification of 'nature'.

Secondly, the contemporary sociology of science undermines some important understandings of the ethically and cognitively distinct character of scientific communities. One effect of the crisis of European liberalism from the 1930s to the early 1950s was the construction of a new exemplary liberal figure – the scientist. In the writings of Michael Polanyi, Karl Popper, Edward Shils and Robert K. Merton, this figure is put forward again and again; see in particular Polanyi's strikingly political account. The scientists' actions are held to be governed by transcendental canons of rational argument.

The communities they form effectively reward the observance and punish the infractions of these canons, by mechanisms which are internal to the community and which must not be disturbed by outside political pressures. These writers effected a reconciliation of scientists with a state whose funds were essential to increasingly capital-intensive research. The accord they made gave scientists much autonomy in deciding what to study and how to study it; it gave them control over the distribution of material rewards and honours; it confirmed their power in the curricula of schools and universities and it gave them a priestly position in public opinion.

The Mertonian programme in the sociology of science was an account of the social conditions of existence of this exemplary community. The post-Mertonian programme has convincingly shown that there are no transcendental norms of rationality, that theory choices are governed by socially and historically contingent rationales. In this it follows Kuhn. The same writers have also undermined the claim that the scientific community is governed by distinct norms of collegial behaviour.

In Michael Mulkay's critique, these 'norms' became an inventory of justifying rhetorics, not internalized and institutionalized principles actually governing behaviour. If there are norms actually governing behaviour among scientists, the post-Mertonians argue, they are the historically contingent *cognitive* norms that make possible the adjudication of truth-claims.

Because the recent sociology of science has displaced these well-established images of how scientists work, I have found it useful in challenging undergraduates' notions of what makes scientists and science special. Scientific knowledge becomes belief constructed in recognizable, familiar human activity. However powerful this demystification might be, it is a local effect confined to tertiary students; the media's implicitly positivist account of science to the public at large remains unchallenged. Moreover the critique itself pulls its punches. Mulkay and others have produced an alternative account, and a more 'realistic' one, of what constitutes the scientific community; but, as in Merton's account, their account privileges 'internal' factors. So cognitive norms replace moral norms as the distinguishing feature of science. They neglect to pose the question of the political relations of this subculture of scientific workers with other institutions.

Much of the radical literature on the politics of science questions whether scientists make a community unto themselves, in a political sense. It cannot make this challenge on *epistemological* grounds. As Mary Hesse has pointed out, in abandoning the notion of a transcendental rationality which distinguishes scientific from non-scientific behaviour, we are not compelled to assume that there can be *no* grounds for attributing to science some special cognitive characteristics; it is just that those grounds are socially and perhaps biologically contingent. In any society and at any time *some* cognitive procedures will be deemed the basis for privileging *some* knowledge as truthful (see Hesse).

We can understand scientific communities to be cognitively different from other subcultures, provided the expression of that difference as 'science/nonscience' is not based on a supposedly transcendental criterion. It is an historically constructed distinction with historically varying contents and criteria. This does not give us reason to believe that any cognitive specificity of those scientific communities entails their autonomy in any other sense, such as moral or political autonomy. We must still inquire as to the articulation of those subcultures with other social structures. We must acknowledge that some cognitive elements of those communities will at times be congruent with beliefs outside the scientific community, and that such congruence may have significance for the otherwise internal history of science.

Science Observed: Tunnel Vision

How have the political implications been taken up by the post-Mertonian programme? As one of its leading figures, Michael Mulkay, is coeditor of *Science Observed*, the book will serve as an indication of how well the programme has fulfilled its critical potential, especially in identifying the social articulation of science with political power.

This is where several of the contributors to this book are very disappointing. They put aside the issue of the articulation of science with other institutions; and they do not consider the class, gender or ethnic composition of the science labour force. They ignore the fact that much of the content of science is determined by the research

priorities of military, business and gender elites. Nor do any authors call attention, as some feminists have recently done, to the likelihood that some cognitive conventions, necessarily arbitrary in their relation to the real, are grounded in a masculinist world view. *Science Observed* is not touched by these political passions.

Five of the nine contributions are particularly blinkered. Lynch, Livingstone and Garfinkel's essay, 'Temporal Order in Laboratory Work', is consciously and deliberately engaged in nothing but the careful description of how some scientists construct, in laboratory activity, that which they say they know. This reveals, for instance, that 'chimpers' interact with the chimpanzees whose behaviour they are studying in ways not always reported in the resulting papers. I am still not sure that this section of their paper is not a superbly deadpan joke; I got that feeling when I came across the phrase 'their animal colleagues'. In the authors' argument, 'chimpers' report some chimp behaviour as 'intelligent', but not the chimps' arguably intelligent behaviour in agreeing to participate in the studies in the first place. Curiously, Garfinkel, Livingstone and Lynch make the same omission. Why did they exclude (with one exception) an account of the behaviour of the scientists towards them as sociological researchers? Should we allow their assumption that the scientists' decision to agree to be studied at very close quarters was not itself part of the scientific behaviour to be studied? Why do scientists consent to such studies? One answer might be that the ethnomethodological account of science cannot be very critical. It accepts the observed activity as given, and even flatters it with its attentions.

Lynch, Livingstone and Garfinkel claim some theoretical significance for their work. They say that their work does not show science's 'foundations' to be in the cognitive norms implicit in particular research activity. Rather, 'various scientific practices compose themselves through vernacular conversations and the ordinariness of embodied disciplinary activities' (p. 208). I am reminded here of other chimps, and typewriters, and resulting reams of Elizabethan verse. Surely this account denudes science too much of the causally effective cognitive traditions which are *among* the conditions of possibility of those 'conversations' and 'disciplinary activities'? But if ethnomethodology is here letting its behaviourist

slip show, it is to some good effect that they give 'activity' logical precedence to meaning. They point out that when scientific papers leave out the mundane laboratory behaviour and report only the 'significant' observations and write up these observations according to an idealized logic and sequence, they are formulating a transcendental account of scientific consciousness. Transcendental rationality is thus an illusion constructed through a professional narrative code seen in the scientific paper.

A similar point is made by Mulkay, Potter and Yearley, who ask us to doubt that scientists' accounts of their actions could be the evidentiary bedrock of sociological accounts of scientific labour. When scientists speak to researchers (or when they write diaries or papers or give speeches) they are constructing accounts, and these constructions are social behaviours which must themselves be explained. Mulkay and his coauthors go carefully through two recent and prestigious sociological studies showing that some actors' accounts are treated *a priori* as more reliable than others.

Mulkay *et at.* say this trust is indefensible. They argue that a discourse analysis of all participants' reports must become the methodological priority of the sociology of scientific behaviour. In effect this blows the whistle on 'virtually all' of the body of post-Mertonian studies. As a contribution to such discourse analysis, they say they have found two 'registers' in scientists' accounts, 'Empirical-impersonal' accounts of a progression of thought occur in justification of a truthful endpoint, or a state of knowledge taken by the speaker to be well-established and defensible. The events of such a narrative are experiments whose accumulative significance is the current state of knowledge. 'Social-contingent' accounts, on the other hand, narrate a history of persons, prejudices, institutions and blind alleys. 'Scientists regularly apply the empiricist and contingent repertoires asymmetrically to account for correct and incorrect belief' (p. 198).

No doubt discourse analysis will make sociological studies of interaction among scientists more defensible, and perhaps less bold in its claims to know the actual sequences of events in passages of science. But Steve Shapin has recently warned against a methodological narcissism that would so inhibit historical reconstruction that it would leave us with only the worried inspection of evidence. It is not

surprising to hear such a warning from a scholar interested more in the construction of causal accounts of social interests' effects on the contents of science, than in a refinement of descriptive protocols which would only plunge us more deeply and exclusively into its micropolitics.

Which Public?

Two contributions by Harry Collins and Karin Knorr-Cetina, try to make a bridge between the refinements in accounts of close-quarters interactions among scientists and the world 'outside' science. Knorr-Cetina's attempt is constrained by her commitment to the view that societies are made up of interacting individuals. The effectivity of structures is invisible to her. Accordingly the 'trans-epistemic connections of scientists' (by which she means, for example, their dependence on granting agencies) are conceptualized as negotiations. A granting agency's goals will have to be turned into concrete research ideas by negotiation between researchers and agency officials, just as their laboratory work is a series of negotiations towards the construction of a shared reality. Only a very limited theory of social power, of the structural determination of the content of scientific effort, will issue from such a behaviourist conception. However, studies of the processes by which research programmes were authoritatively constructed still have an intrinsic interest.

Collins's paper sketches the possibility that in explaining the closure of scientific debates – closure being the social behaviour by which controversy ceases and a particular claim is accepted – we must take the public relations of protagonists into account. He mentions the 'superior rhetorical resources' of industrial companies compared with university departments, and suggests that the interests of firms marketing technology whose reliability is assessed through a current body of knowledge may impart a conservative bias to the resolution of theoretical controversy around that body of knowledge. This usefully extends our understanding of the possible ways in which social interests may cause scientific knowledge to have this or that content. I would add that if these rhetorical resources are to be taken seriously, then we must think also about who

are the public on which they are employed. The political activity of organizations using scientific arguments might have as one of its objectives the favouring of one definition of the public to whom they have a responsibility. All definitions of 'the public' are politically tendentious.

And if there are variable publics for scientific controversy, then there will be variable publics for sociologists of science who intervene. Collins looks at possible public roles for sociologists of science. Firstly, he says sociologists could develop new terms for comparing passages of scientific work, to assist granting agencies; the new terms would not be quantitative (numbers of publications, cost per paper, honours earned) but would try to deal with the cognitive aspects. 'We are going to have to think in terms such as "revolutionary science", "low risk science", "extraordinary science", "science done in order to maintain or develop a body of tacit knowledge", "replication of others' findings" and so forth' (p. 98).

What is the public to whom these discriminations will become meaningful? Collins does not say. But he does think more sociologically about the public in his discussion of the sociologists' intervention in the closure of scientific controversy. Closure may be effected in different ways before different publics, he suggests. For instance 'we should be asking if unchallenged forensic evidence which is taken to justify a prison sentence would be taken to justify a radical conclusion *within* science'. But, again, to whom can the sociologists of science address such an important question? Courts have not yet demanded sociological analyses of the rhetorics and institutional procedures which establish the credibility of expert witnesses. There is an odd thought which arises from Collins's paper – that sociologists could become the arbiters of scientific authority by defining some closure devices as being more legitimate than others. Such a judicial use of the sociology of science is unlikely, but perhaps the sociology of science could become one of the resources that adversaries use to undermine one another's scientific experts.

While Collins leaves hanging the issue of the nature of the public addressed by the sociology of science, Steve Woolgar seems to assume a narrow public of sociological *cognoscenti* in his 'Irony in the Social Study of Science'. He points to an unresolved tension in the discipline between realism and relativism – whether the socially

constructed content of scientific knowledge is in any way referrable to a real of which that knowledge is a mediation, or whether, rather, socially constructed knowledge of the real is the only real that can be referred to. Among sociologists of science and their readers, he says, there is a tacit agreement not to raise this issue; the sociology of science is accordingly an ironic discourse. The impact of a sociological account of a passage of science on a public of sociologists of science will not be to discredit the knowledge claims, as they know that the same 'job' could be done on *any* scientific work.

But what about the lay public? Woolgar does not make any distinctions between publics; he does not discuss the social composition of the public at all. The irony for him arises from the likelihood that readers of science studies hold simultaneously to realist and relativist epistemologies. Perhaps this is so, but I doubt that relativism has many adherents among the lay public. Until they learn to be at least half relativist in their thinking, their perception of a knowledge shown to have social causes will be that it is discredited as science. If this is so, then sociologists of science are faced with two important political choices: which knowledges to account for sociologically, and which publics to address. The public discussion of the content of science takes place within a field of political conflict which gives science, and sociological deconstructions of it, shifting (rather than intrinsic) political meanings. Woolgar's elegant sense of the paradox of his discipline is blind to the existence of the field of shifting political choices in which commentators on science find themselves.

This is happily not so for Daryl Chubin and Sol Restivo, who have studied the US 'war on cancer'. Like Collins they see some role for sociologists in slowing down the closure of scientific controversies, or making that closure occur in full knowledge of the wider political implications of contending claims to truthfulness or fruitfulness. Social analysis could also open peer review procedures to public scrutiny to make allocations of research funds less the mysterious prerogative of closed elites. A sociologist is an active citizen; his/her reflexivity is more than 'scholarly rumination', they affirm. They aspire to struggle over science policy, to create a climate of less certainty in resource allocation by exposing the value-laden character of all parties' points of view. But they are uncertain as to how policy-

makers can be made to accept the critical interventions of such 'metascience'. They seem to me to assume that the corridors of power will happily admit their provocations and adjudications. It is a rationalist fallacy to assume that criticism, not backed by political muscle, will be welcomed to the policy-making process for its own sake.

Causal Models

So far in this review I have drawn attention to two political limitations of the contemporary sociology of science. Some strands of research reproduce in new form a conception of science as an autonomous subculture; their account is critical only in the sense of being ironic about previous rationalist and empiricist accounts of what made that world distinctive in ethos and cognitive style. Others, who have a sense of the connections between powerful institutions and the socially constructed contents of science, have only just begun to think hard about the political utility of their work to the nonacademic publics which have a vital stake in the governing of science. Two papers which I have not yet discussed, one by Barry Barnes, the other by Bruno Latour, are quite different from the others in the way they are implicated in these two issues.

Actually, Barnes's essay 'On the Conventional Character of Knowledge and Cognition' is a disappointment because it repeats the argument he has published elsewhere: that knowledge is necessarily 'interested' activity, without dealing with the problems of the term 'interest'. Two other contributors, Woolgar and Yearley, have published carefully considered critiques of Barnes's (and his colleagues') endeavour to explain the content of science by reference to social interests. The 'Edinburgh school' (as Barnes *et al.* are known) case studies of the interests active in scientific disputes are both exciting and vulnerable. They have gone as far as anyone in connecting the content of science with the structure of social power, though in case studies of the past, not of the present. But the logic of their arguments is vulnerable to Woolgar's and Yearley's critiques of circularity – in the nomination of the social agents who are said to have interests, and in the attribution of the (extrascientific) substance of those interests. On the best academic grounds, I would

award the points to these critics; however, as Donald McKenzie commented in defence of his own work, the critics are setting standards of the logical adequacy of causal accounts which are rarely met in historical and social studies. To take these standards to heart would be to withdraw from the goal of explanation, to that of description, at the expense of a political account of science – an account of a powerful cultural form whose contents bear the traces of the wider structures of power in the society (see Barnes, McKenzie, Woolgar, Yearley).

McKenzie said he was willing to live with his less rigorous but more interesting research programme. I think this also implies (and I agree with him) that he could live with the less refined expectations of a nonspecialist public that may want to mobilize around a political account of science which is admittedly tendentious. Woolgar and Yearley assume in their writings that the cognitive norms they are applying (i.e. their notions of causal adequacy) are transcendentally relevant, whereas they are part of the cultural practice of a specifically academic interest in the social study of science.

Bruno Latour's paper deals with the relations of determination between social interests and the content of science by saying that the causality runs the other way. In a case study of Pasteur's relationship to the French state's government of agriculture, he argues that the world was refashioned according to the requirements and findings of Pasteur's laboratory research. Laboratories are a 'source of political strength, strength which is *not* explained by any cognitive or social peculiarities'. The power of laboratories is that they develop devices for the writing down of the 'findings' to which they give rise. Laboratories make phenomena visible and inscribable. Within the technologies of administration of society at large, the inscribable and measurable is hard currency. Pasteur in his laboratory had superior resources for inscribing the proliferation of anthrax microbes, superior to the resources of officials out on the farms where the real epidemic was occurring; the artificiality of the laboratory is what makes it superior to 'in the field'.

> The specificity of science is not to be found in cognitive, social or psychological qualities, but in the special construction of laboratories in a manner which reverses the scale of phenomena so as to make things

> readable and then accelerates the frequency of trials, allowing many mistakes to be made and registered.(p. 165)

Accordingly, the scientist seeks to extend the conditions in the laboratory to the world 'outside' it, to reconstruct the world in the laboratory's image, so making possible the widespread 'reproduction of favourable laboratory practices'. The standardization of weights and measures is one instance of this. So science works to eliminate 'the outside' of science, by refashioning that 'outside' in its own image, replicating laboratory life at large. The universal applicability of the scientist's results has to be socially constructed by reconstructing society. Moreover these reconstructions are not a derivative, through science, of the economic power which has science as its instrument; they issue from science itself. It is a source of 'unpredictable displacement'. The study of laboratories is thus the study of central and determining political processes.

The phrase 'technological determinism' nagged at me as I read Latour's fascinating paper, which I found the most interesting in this collection. But perhaps that phrase refers, not to an easily recognized historiographic fallacy, but to an aspect of reality as we know it, to a peculiarly modern set of social determinations – the extension of the domain of a reified and predictable nature. We are still speaking here of a social phenomenon, but one whose idiom is technology, as if technology were only the expressing of Nature in humanly usable form.

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NIELS BOHR: 'Mr Atoms for Peace'

Mike Barnett

Review of Ruth Moore, *Niels Bohr: The Man, His Science and the World They Changed*, MIT Press, 1966 hb, 1985 pb, 436 pages, £10.50

In 1956, an 'Atoms for Peace' exhibition toured the UK in advance of the opening – with many a pretty speech – of the Calder Hall military reactor. At the time, few except the instigators of this charade were aware of the true rationale for bringing onstream the world's 'first civil nuclear power station'. In 1957 the Ford Foundation established the Atoms for Peace Awards. The first recipient of the $75,000 prize was the Danish physicist Niels Bohr, dubbed 'Mr Atoms for Peace' by an American newspaper. It is difficult to believe that Bohr accepted this prize in a cynical spirit, but it is also hard to avoid the conclusion that he was, in effect, duped by those who intended 'Atoms for Peace' to be the cover for 'Atoms for War'.

The sad irony of this is that it was almost certainly Bohr himself who had first suggested that people might be reassured by the notion that every mushroom cloud had a silver lining in the form of cheap electricity. Early in 1945, in a memorandum to the American president, he had written:

> Indeed it need hardly be stressed how fortunate in every respect it would be if, at the same time the world [learns] of the formidable destructive power which has come into human hands, it could be told that the great scientific and technical advance has been helpful in creating a solid foundation for a future peaceful cooperation between nations.

To most contemporary physicists, Bohr is no more than a name encountered in syllabuses, textbooks and exam papers; his involvement in nuclear politics will come as a surprise. Ruth Moore's com-

prehensive 'popular' biography, reissued in paperback to coincide with Bohr's centenary in 1985, goes some way towards filling in the details of his life.

Bohr has a secure place in elementary textbooks: 'In 1913, N. Bohr proposed a planetary model for the atom...' There is no doubt that as head, from 1920, of the highly successful Copenhagen Institute of Theoretical Physics, he exerted a profound influence on the development of quantum physics. He also played an important part in formulating the theory of nuclear fission immediately before the Second World War and, on escaping from occupied Denmark in 1943, was drawn into the A-bomb project. Foreseeing a nuclear arms race, he campaigned from 1944 onwards for international cooperation in the development of atomic energy and in arms control. This meant, in effect, telling the USSR without delay about the existence and likely success of the Manhattan Project (they knew already!). Churchill would have none of it, even though no engineering details were to be disclosed.

The book turns out to be a very mixed blessing. It is highly detailed, particularly about comings and goings between Copenhagen, Cambridge, Göttingen, Manchester, Princeton and points west. Thus it cannot fail to convey that many of the syllabus names (Heisenberg, Pauli, Einstein, Schrodinger, etc....) knew each other, often worked together and could even disagree profoundly on basic conceptual issues. The author attempts to interweave into the narrative what the publisher describes as an 'introduction to nuclear physics for the nonscientist'. This is reasonably well done – I understood most (but not all) of it.

On the debit side, the author clearly espouses the 'let us now praise famous men' style of biography – Niels Bohr, secular saint, superstar and hereditary genius – with the result that the reader has to endure a cruel and unusual quantity of awestruck gush. For example, '...The discoveries were piling up – the proton, the neutron, the positron, artificial radioactivity, the transmutation of matter and the powerful new artillery for smashing still further into that citadel of the atom, the nucleus. Physics was as alive as an excited atom.'

The uncritical adulation of theoretical physics and physicists is pushed to great lengths. Of the Bohr-Einstein dialogues we learn

that 'There was no agreement, no complete meeting of the two greatest minds of the century...only Socrates debating his theories with the great Eleatic philosophers, Parmenides and Zeno, had explored reality as profoundly and subtly.'

Now one might be inclined to dismiss this as harmless, if silly, hyperbole, but it engenders an absurd historical perspective:

> As Bohr arrived at the Berlin physics building, Planck and Albert Einstein came forward to greet him...Three of the four men who were shaping and would shape the world for at least the next century – only Rutherford was missing – stood talking together...

We should recall what happened to the 'big three' the minute they dared to step out of line – Planck treated contemptuously by Hitler, Einstein ignored by the Truman administration and Bohr, as the present book recounts without any apparent sense of internal inconsistency, labelled as a potential subversive by Churchill and Roosevelt. The infamous 'Aide-Mémoire of the Conversation between the President and the Prime Minister at Hyde Park 19th September 1944' contains just three clauses, the third of which reads '3. Enquiries should be made regarding the activities of Professor Bohr and steps taken to ensure that he is responsible for no leakage of information, particularly to the Russians.'

On balance, I would rather this book were read than remain unread, though much more for the matter than the manner. The historical and social settings – a series of *Reader's Digest*-style cardboard backdrops to the unfolding drama of great men and great minds – are presented with a remorseless superficiality which ill befits a deadly serious subject. Bohr has been the subject of several books; more could usefully be written. The one most relevant to today would be essentially tragic, recounting the story of a celebrated and justly admired scientist, with a highly developed sense of social responsibility, who came, in all innocence, to be the unwitting begetter of the sinister 'Atoms for Peace' con-trick.

This perspective is wholly lacking from Ruth Moore's book, though admittedly it was first published in 1966, before the facts of nuclear life were widely known. However, no form of revision or re-evaluation appears to have been undertaken. This leaves the passages recording Bohr's advocacy and organizing role in launch-

ing Denmark's nuclear power programme without so much as a footnote explaining how and why this programme was later abandoned.

However, given our present anxieties, it is difficult for any account of the development of nuclear science, however historically unserious, to be less than thought-provoking. This must be particularly true for the increasing numbers of scientists who are, at long last, starting to question the propriety of accepting military funding. The lesson from Bohr's experience is: when it comes to supping with the devil, there is no spoon sufficiently long – if you compromise, you will end up compromised.

Cultural *Critique*

American Representations of Vietnam

No. 3, Spring 1986

Scholars and critics in many disciplines have begun to study the great variety of documents, personal accounts, and stylized representations of America's war with Vietnam. In many ways, it was a war waged by various media as well as in actual combat, and the aftermath of this war continues to haunt us with difficult questions about how the representation of another culture shapes foreign policy and public opinion. This special issue of *Cultural Critique* offers various approaches to these practical and theoretical issues in the American representations of the Vietnam War.

John Carlos Rowe and Richard Berg, *Vietnam in the Academy: Observations on the Rhetoric of Scholarship*

John Carlos Rowe, *Documentary Styles in the American Representation of Vietnam*

Michael Clark, *Snapshots: Remembering Vietnam*

Claudia Springer, *Military Propaganda: Defense Department Films from World War II to Vietnam*

Richard Berg, *Coming Home: The Veteran on Television and in Film*

Noam Chomsky, *Reflections on the Indochina Wars*

Philip Kuberski, *Genres of Vietnam*

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W. D. Ehrhart, Poetry

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SALVADOR LURIA

Erwin Fleissner

Review of Salvador Luria, *A Slot Machine, a Broken Test Tube*, NY, Harper & Row, 1984

In 1937 a young Italian doctor made a trip from Turin to Rome to spend a year studying physics. He thought of physics as 'a romantic landscape whose magic was enhanced by my almost complete lack of understanding'. Although lacking any real training in physics, he was excited by developments in elementary particle theory and considered changing careers. In contemplating such a precipitous step, he was seized with indecision: 'In the gloomy light of the blue night lamps of the train, I realized that fear is part of the adventure, and fell asleep.' The young man, Salvador Luria, was not destined to become a physicist. He became, instead, one of the great biologists of the century, one of the progenitors of molecular biology and its offspring, genetic engineering.

I first met Luria in 1975, when he was in his early sixties. I was on a team of visiting scientists reviewing an NIH grant to the MIT Center for Cancer Research, of which he was the director. What struck one immediately about him was his simplicity and directness. He showed no inclination to use his considerable prestige as an armour against sharing his thoughts, even in this formality-strewn situation. One could see his pride in the excellent group of young scientists that he had assembled, and his excitement about what they might (and later did) accomplish. Speaking of one of them, he said, characteristically, 'He has a deep love of science.' This comment about someone whom other acquaintances considered as something of a brash young *enfant terrible*, was revealing of what mattered most for Luria about an individual engaged in research. His feelings about science – its beauty, its frustrations – are passionate; they persistently break through that part of his nature which is more reserved.

A New Experimental System

Luria was armed in his youthful excursion to Rome with an introduction to the reigning intellect of Italian physics, Enrico Fermi. Fermi was one of the new breed of atomic physicists – direct, informal, intolerant of nonsense, hospitable to young people. Luria was given an assistant's job, 'garzone' – a rather menial position, but appropriate to his inexperience – with an outstanding experimentalist in the group. Meanwhile he attended undergraduate classes in physics. One day when the train broke down on the way to the university, as it frequently did, he fell into conversation with a biologist, Geo Rita, who was studying bacteriophages, the viruses of bacteria. In Rita's laboratory Luria saw the ability of phage to destroy a culture of bacteria within minutes, while the phage reproduced. Thus he found his experimental system.

The simplification afforded by studying elementary organisms, parasites of microscopic hosts, was combined with the precision of handling huge numbers, billions of viruses and bacteria. From his studies of this system in the next fifteen years came the discovery that bacteria undergo spontaneous mutation (to phage resistance), a key to molecular genetics, and the demonstration that bacteria can specifically cleave the DNA of incoming phage. The latter finding led, eventually, to the isolation of the cleavage enzymes, so-called restriction enzymes, which are central to the practice of genetic engineering. But these things happened on another continent, at universities in the American mid-West, after Luria had fled from the fascist grip tightening around his native Italy.

The quantitative biology of minute organisms was paradigmatic, in Thomas Kuhn's sense, of the coming revolution in biology. Luria's love affair with bacteriophage was catalysed by his physics mentor, who gave him some new papers on the interaction of radiation with living matter, written by Max Delbrück, a young German physicist. Delbrück and his colleagues thought that new physical principles might be uncovered in living systems.[1] These hopes were not fulfilled. Instead, molecular biology sprang, fully armed, from the 'old' physics of quantum chemistry.

Luria's autobiography (*A Slot Machine, a Broken Test Tube*) recounts these events with considerable candour. Any such self-revelation by a modern biologist inevitably invites comparison with

Watson's *The Double Helix*. Luria's self-revelation is less complete than Watson's, not so much in content as in style. Where Watson invites us into the mind of a shy yet intellectually confident young man, Luria observes himself, as it were, with a psychoanalyst's detachment. His confessional statements ring true, but they do not have the postponed adolescent quality Watson's have, perhaps because they seem so thoroughly understood. Watson wrote in his forties, while Luria writes from the vantage point of seventy, looking back on a full career.

The sheltered childhood which Luria recalls was conducive more to hypochondria than to genius. His father favoured medicine as a remunerative career. His mother communicated more of the excitement in intellectual things, but was lost to a morphine addiction after a serious illness. From an anti-fascist teacher-friend devoted to physics, Luria learned how to make his escape from a home atmosphere alternately bourgeois and fearful of physical ailments. His elder brother was entrapped by this atmosphere; Luria eloped with his scarcely realistic plan to become a nuclear physicist, and took the train to Rome.

The tightening of anti-Semitic regulations in Italy in 1938-9 led him to Paris, where he pursued his newfound phage addiction and attempted to apply Delbrück's target theory for radiation to phage particles. Paris offered a new freedom from family-inspired social and sexual inhibitions, but he did not exploit the opportunity, instead cultivating more cerebral interests in music, painting and literature (also undernourished by his family) and working hard in the laboratory. Fleeing Paris before the Nazi advance, he wandered in picaresque youthful invulnerability through the rural French countryside, dodging German warplanes, meeting other displaced Italian intellectuals and eventually finding his way via Marseilles and Lisbon to America. There, equipped with an introduction from Fermi, he established himself at Columbia University.

Soon he and Delbrück, also a refugee, had their first face-to-face discussion about bacteriophage. They reconvened that summer to work together at Cold Spring Harbor on Long Island. Cold Spring Harbor was more a haven for bacteriologists than its famous counterpart at Woods Hole. The summer community occupied a few ramshackle houses on an inlet of Long Island Sound, but it

already included two future Nobelists, Al Hershey (co-laureate with Luria and Delbrück) and Barbara McClintock, the maize geneticist. The informality was perfect for phage work, for which a few petri dishes and a primitive incubator were the basic equipment.

Luria and Delbrück were complementary personalities. Their collaboration represented one of those rare sum-greater-than-parts interactions in research, of which Watson and Crick are the cliché example in modern biology. Such interactions frequently involve a more introverted personality (as Watson, Luria) and an extrovert (Delbrück, Crick). Delbrück needled Luria about his Italian accent and seized the intellectual initiative; Luria patiently uncovered the biological phenomena.

The first electromicrographs of phage particles, obtained by Luria and T. Anderson in 1942, make it possible at last to see the tiny beasts. With this fresh success, Luria was able to obtain a university post in Indiana. There he had his momentous encounter with a slot machine (at a faculty party), which suggested to him a statistical method for proving that bacteria mutate spontaneously. He passed the data immediately to Delbrück (then at Vanderbilt University), who replied tersely, 'I believe you have something important', and contributed a mathematical theory. The 'Luria-Delbrück fluctuation test' for spontaneous genetic changes – as opposed to Lamarckian changes induced by the environment – became a cornerstone of microbiological Darwinism. By now a biologist of considerable repute, Luria stayed more than a decade longer in the mid-West, training disciples such as Watson and R. Dulbecco (both future Nobelists) before moving to MIT in the late 1950s. The move back East was partly determined by his wife's career, as she was offered a position as a psychologist in Boston.

Politics, Inner Conflicts

As a committed feminist in the context of his socialist convictions, Luria shared with his wife household tasks such as cooking meals and washing the dishes. She had an outspoken, direct personality, complementing his. She broke through his layer of reserve; his earlier romantic attachments had involved overidealized women, far off, unattainable.

At MIT he became more politically active. An active involvement in left-wing politics, with an existentialist bent, had already started in Paris; now he became a leader in the academic resistance to the Vietnam War and a series of other causes. His attitudes were low-key and matter-of-fact, but the commitment was unremitting. He says that all of his closest friends have been socialists.[2] In the 1960s he was one of the originators of the newspaper advertisement as political strategy. In such advertisements the scientific prestige of the MIT faculty was a powerful weapon when applied to public questions involving technology. More recently he and other Harvard and MIT scientists have opposed the doctrines of sociobiology promulgated by Harvard's E.O. Wilson.

In a striking personal revelation, Luria describes his struggle against recurrent depression during much of his adult life. The amount of energy siphoned off from career and family by these bouts must have been prodigious, but they never brought him to a full stop. He continued active research, teaching and writing throughout. At MIT he and his wife hosted regular Sunday-night informal discussions of literature for students, choosing themes such as 'inner conflict' or 'human frailty'. After his Nobel Prize (1969), he moved more into administration, while continuing his other activities.

He credits his now decade-long remission from depression to the effective use of drugs, particularly lithium. He had tried psychotherapy of all kinds in the preceding twenty-five years, but it proved essentially worthless. The indictment of 'talk therapy' which he delivers is concise and, as related to his personal experience, devastating. Lithium appears to have been the way out of the maze for him. It is possible that his intensely analytical temperament made verbal therapy less effective in dislodging and bringing to light deep-seated feelings. What he tells us of his early life, his strong ambition poised against intense feelings of physical insecurity and self-doubt, certainly suggests sources of depression other than the merely biochemical. The mutual interdependence of the psychological and the biochemical is, of course, not denied by Luria. Like his friend Jacques Monod, he sees the appropriate human response to biological determinism as existentialist: there is no escape from the responsibility for conscious decision-making, regardless of how one's genes are arranged.

Luria early acquired a sense of inadequacy at sports and in situations suggestive of masculine competition or aggression (he says the smell of a gymnasium makes him ill). He links this to a dislike of outdoor biology of the hearty, specimen-gathering variety, and, by association, a scepticism about holistic approaches to organisms. Reductionism has been the battle cry of victorious experimental biology, in his view, and the opposite tendency is pious nonsense. Oddly, he also dislikes scientific gadgetry, shuns computers, abhors TV. He is a classicist and a purist in his approach to science, as well as a reductionist, and something of a patrician in his cultural tastes.

The central experience in research is for him the confrontation of a problem – the enigma requiring a solution. Any means may then be used to find the solution. (He used the electron microscope – a supreme piece of gadgetry – in visualizing phage with Anderson.) But the chief means is a plainly spelled-out logic. Perhaps logic dictates a repetition of the experiment with a small perturbation, an extra twist to probe why the phage won't grow, or why the bacteria aren't killed. The step-by-step accretion of information may then lead to a new hypothesis, e.g., that phage are restricted by their bacterial hosts, or that, conversely, phage modify their hosts. The logical elements become the phage particles themselves, and the events in their life cycle become parts of a Darwinian drama keyed to survival. Temperamentally Luria seems more at home with the Anglo-Saxon tradition in biology. Perhaps that is also why he found the plain-spokenness of the American mid-West so congenial.

He does not want to put his name on a student's publication unless he has made a tangible contribution to the work – again a departure from the Continental tradition. His laboratory appears to have been a reasonably relaxed place for students and associates to work. Luria consistently avoided competing in the most glamorous areas opened up by phage research: gene replication and gene expression and regulation. He remained faithful to the tradition of phage biology that he, Delbrück and the laconic A. Hershey established. He deviated only to study a more primitive sort of particle, a sort of incompletely evolved virus, the colicin factor. The research depended more on some long-term associates as his administrative role grew. The impression is that these relationships – one in particular – have been mutually rewarding, without the

customary exploitative aspect. This may reflect the small size of his lab, like the early phage labs, where professor and students could meet on an equal footing. Luria remarks that, in contrast to Delbrück, he has not had the knack of forming very informal close associations with students. He attributes this to his persisting personal insecurity, which made such close relationships uncomfortable.

Nevertheless, one feels that the lack of cant and any type of posing on his part, coupled with his sense of humour and love of science, must have made a deep impression on generations of students exposed to his classroom teaching, as well as to his presence in the laboratory. This sort of modesty and intellectual dedication has become rare among the molecular biologists who flock to genetic engineering companies, eagerly accumulating stock options and consultant fees. Luria would be the last to adopt a holier-than-thou attitude towards such colleagues; one suspects he would simply find their pecuniary interests boring in comparison with unravelling a good puzzle in the laboratory or perhaps taking on the Establishment on a juicy political issue.

Notes

1. 'From Delbrück's general picture of the nereditary substance it emerges that living matter, while not eluding the "laws of physics' as established up to date, is likely to involve "other laws of physics" hitherto unknown which, however, once they have been revealed will form just as integral a part of this science as the former.' – E. Schrödinger, *What is Life?*, quoted by G. Stent in J. Cairns, G. Stent, and J.D. Watson, eds, *Phage and the Origins of Molecular Biology*, Long Island, New York, Cold Spring Harbor Laboratory, 1966.

2. Luria confirms an anecdote related by Watson in *The Double Helix*, in which the crucial move of Watson's post-doctoral work from Copenhagen to Cambridge, where he linked up with Crick, depended on Luria's having become friendly with John Kendrew, a Cambridge biophysicist who shared his socialist convictions.

ENERGY FUTURES

Les Levidow

Review of Andy Porter, Martin Spence, Roy Thompson, *The Energy Fix: The Politics of Power*, Pluto, 1986, 191 pages, £5.95 pb

Nuclear power, originally promoted as an energy source 'too cheap to meter', has come to leave a trail of death and destruction too expensive ever to measure in money terms.

After the Three Mile Island disaster, people still remained willing to consider that nuclear power could be made acceptably safe. Although sceptical, they tended to feel resigned: 'The experts disagree; who are we to judge?'

Since Chernobyl, however, the prevailing mood has shifted away from feeling reassured by experts, much less by politicians. Now, no benefit seems worth the risk of another Chernobyl closer to home; indeed, no one seems safe from a wandering radioactive cloud. Furthermore, the Chernobyl syndrome has intensified suspicions about nuclear waste dumping and about covered-up 'incidents' at other nuclear plants.

Of course government and industry will continue to attempt nuclear expansion anyway, if they think they can get away with it. But they have been put on the defensive, especially regarding non-nuclear alternatives, which are becoming a focus for public debate. What, then, is meant by an alternative? That depends upon one's politics. Unfortunately, anti-nuclear approaches have tended to treat nuclear power as a technical (albeit evil) contribution to the nation's energy supply; they have proposed alternative techniques for closing a potential energy gap, as if the problem were one of matching supply and demand in cleaner, safer and/or cheaper ways. Largely ignoring the political role of energy production and consumption, the 'alternative energy' debate contains its own traps of technical mystification, if only by default.

The Energy Fix

By contrast *The Energy Fix* analyses how Britain's main energy sources are bound up with class domination. It identifies the state's priority for energy policy: the need by private enterprise for a flexible energy source, secure from disruption at home or abroad. That priority has meant an emphasis on electricity generation, which entails enormous energy losses by, for example, converting fuels first into electricity, transmitting it and then converting it back again into heat. In that sense electricity generation is the least efficient way to use most primary energy sources (fuels such as coals or oil).

Yet the electricity supply industry is Britain's largest single consumer (35 per cent) of those energy sources; indeed, the proportion has been rising, along with the total amount of primary energy needed to feed the industry, despite the recession. Why? As compared to a more direct use of primary energy, electricity generation offers industry a more flexible, universally applicable form of energy, better suited to the unpredictable course of competition among firms. And an installed capacity far in excess of peak demand provides political security, such as when extra oil-fired generation helped to rescue the government during the 1984-5 miners' strike.

The Energy Fix shows historically how capitalist priorities – political security and economic profitability – have guided the selection and development of various energy sources. For example, the cheap and apparently secure oil supplies from the Middle East – one of the spoils of World War 2 – encouraged the British state to run down the mining industry and so reduce reliance upon rebellious miners (sound familiar?). Meanwhile it made enormous investments to develop nuclear power, both for its military benefits and as an export commodity, albeit unsuccessful against US competition. Eventually the 1973-4 OPEC oil price increases led the Labour government to agree to new mining investment, while continuing to expand nuclear power – both of which, a decade later, the Tory government used to defeat the miners' strike against the closure of so-called 'uneconomic' pits. At the same time, the North Sea oil boom depended upon OPEC's political price for oil, while, ironically, the government used the resulting income to subsidize an

economic policy supposedly dedicated to 'free market' principles.

As well as outlining the capitalist policies of the Labour and Tory governments, this book criticizes the Labour Party-TUC proposals for expanding coal mining, nuclear power and energy conservation. If implemented, this would result in a glut of expensively produced energy. By attempting to preserve all existing jobs, at least on paper, such proposals evade the crucial political choices, especially the elementary point that developing some energy options precludes others.

Also criticized are various environmentalist studies which propose renewable energy sources and/or energy conservation. Like Green politics in general, such studies tend to attribute our energy problems to 'industrialism' or to economic growth as such, rather than to capitalist priorities.

By contrast, the book locates proposals for alternatives within the larger perspective of socialist reindustrialization. That is both the strength and the weakness of its approach, as we shall see. Its detailed proposals, based on social need rather than profit or domination, call for new kinds of investment. They would expand some (non-nuclear) energy sources currently in use, with innovations such as combined heat-and-power, as well as renewable sources and conservation.

Forces for Change

Who would implement all that? There is no expectation that a future government would simply initiate such anti-capitalist planning. (However, having repeatedly criticized past governments' lack of overall economic planning, the book seems not to recognize those governments' policies as authentic capitalist planning, as if planning were inherently socialist.)

For the necessary impetus, the authors place their hopes in a growing unity of environmentalist and labour movement 'forces for change', which could ally with similar forces in Europe to develop an internationalist approach. Assessing the recent experience of workers' plans and popular planning, which lacked sufficient protection from market pressures, they note that the imperative for profitability has led new enterprises to 'either the transformation of

workers into successful small businessmen/women, or the failure of the enterprise and the resulting demoralization of the workforce' (p.147).

Therefore they look to central government backing to protect their proposed alternative energy policies from market pressures. Despite the clear record of every past Labour government, they somehow expect the next one to 'help initiate a radical restructuring of society, if it supports and extends working-class power' (p.153). Internationally, they expect it 'to exploit the structure of the capitalist market in order to create a widening international network of anti-capitalist practice' (p.161).

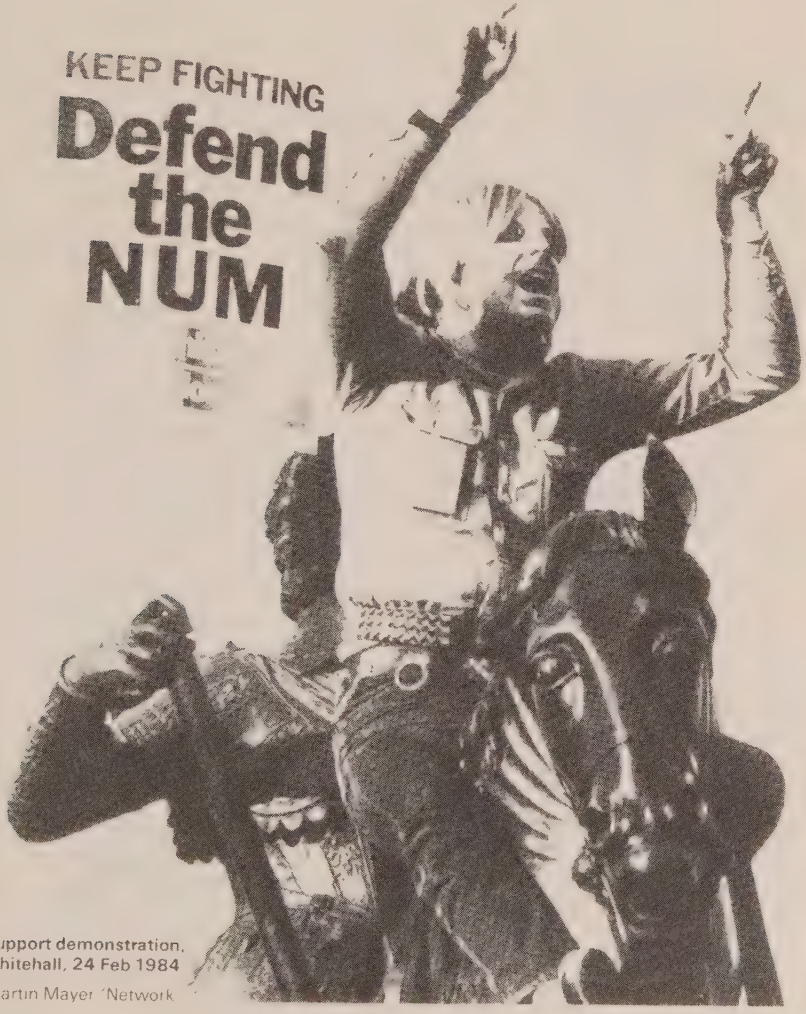
Although their wishful thinking does not invalidate their detailed proposals, it does reveal that their critique has fallen short. Surely Labour, no less than the Conservative Party, is committed to a high-tech reindustrialization for enhanced international competition; those priorities require energy-intensive (especially electricity-intensive) technologies which replace skilled human labour with mechanical energy plus more menial labour. Although the authors mention the problem of deskilling, they do not connect automation to capitalist criteria for energy sources and uses.

If we consider the state's role as guardian of capitalist restructuring at home and of British investments abroad, then clearly the authors' proposed political alliance must expect future governments to resist, not protect, anti-capitalist energy schemes. If we are to get beyond the technical fix mentality, implicitly criticized in the book's title, then we will have to consider a somewhat different starting point. It would be realistic to expect an energy policy to give priority to social needs only if implemented by a popular movement willing to reappropriate the resources (labour, raw materials, equipment) now managed for capital by the energy industry. That would also mean a movement capable of defending itself against the state for 'violating private property rights' – that is, capital's right to manage.

As a recent down-to-earth example, the defeat of the 1984-5 miners' strike was at the same time a victory for capitalist energy policies, at several levels. Not only did it weaken the National Union of Mineworkers, but it validated existing priorities for state subsidy and investment: expanding nuclear power, while automat-

ing pits to enhance managerial control, while largely neglecting renewable energy sources and conservation.

In retrospect, we must ask ourselves: What kind of political strategy, what kind of movement, would have been necessary to reverse the defeat of that strike? What lessons should we draw for how to win anti-capitalist energy policies? These realistic questions have hardly been posed, much less answered.



Support demonstration, Whitehall, 24 Feb 1984
Martin Mayer 'Network

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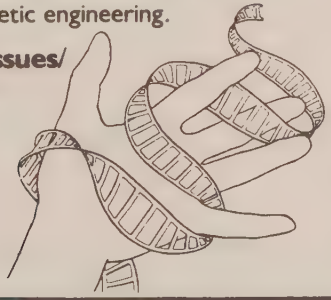

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Marleen Barr. "The Females Do the Fathering": James Tiptree's Male Matriarchs and Adult Human Gametes ... 42
Stephen M. Fjellman. Prescience and Power: *God Emperor of Dune* and the Intellectuals 50
Nachman Ben-Yehuda. Sociological Reflections on the History of Science Fiction in Israel 64

REVIEW-ARTICLES
Susan Gubar. Feminism and Utopia 79
I.F. Clarke. *Dorking* Revisited 84

BOOKS IN REVIEW
Chronic Adolescence (Linda Leith) 87
Science Fiction and Fantasy Film: Shadow and Substance (Andrew Gordon) 89
Morris as Prophet (Patrick Parrinder) 92
Interpretatively Sound, Theoretically Weak (Linda Leith) 92
A Descent into Respectability? (David Ketterer) 94
One Man's Canon (Veronica Hollinger) 95

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